

Essential but Not Sufficient: Internet Essentials and the Barriers to Financial Inclusion

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Abstract

We study how internet access affects financial inclusion among low-income US households using Comcast’s Internet Essentials program. Combining a triple-difference design with data from the ACS, CPS, Experian credit bureau, and IRS, we find that subsidized broadband increases employment, expands credit market participation from search through acquisition to management, and increases savings. Equity market participation does not respond. The pattern is consistent across four independent data sources and robust to a range of sensitivity analyses. We interpret the results through a distinction between frictional barriers that technology can reduce and structural barriers that persist regardless of information costs.

Keywords: Financial inclusion, internet access, digital divide, credit markets, household finance

JEL Codes: G50, G51, D83, L96, I38

1 Introduction

As financial services migrate online, it is natural to assume that internet access is central to financial inclusion. The chief of the Federal Communications Commission’s Office of Economics and Analytics argues as much: “to successfully expand financial inclusion in the United States requires enhanced internet access” (McHenry, Goldberg, Lewis, Carlson, and Mehta, 2017). Successive US administrations have acted on the premise, investing billions in subsidized broadband, most recently through the Affordable Connectivity Program (ACP), which served over 23 million households before its funding expired in May 2024.

But financial activities differ in what actually keeps households out. Some barriers are *frictional*: the search, information, and transaction costs of finding and managing a product, and recede when information access gets easier. Others are *structural*: the discretionary wealth, income stability, and qualification a product requires, and do not immediately decrease with information access (Peress, 2005; Vissing-Jørgensen, 2002; Campbell, 2006). Internet access can relax the first and not the second, so its effect on inclusion should vary, activity by activity, with the barrier that binds. Does removing these frictional barriers translate into broad-based financial inclusion, or do deeper barriers persist even after households gain digital access?

We study this question using Comcast’s Internet Essentials, a subsidized-broadband program for low-income households. It generates three sources of identifying variation: geographic differences in Comcast’s cable footprint, household eligibility based on income and the presence of school-age children, and the program’s national launch in 2011. We combine this triple-difference design with data from the American Community Survey, Current Population Survey, Experian credit bureau records, and IRS tax filings to trace the effects of internet access across employment, savings, credit markets, and equity market participation.

Consider two families in Houston, the second-largest city for subsidized internet enrollment, each earning \$22,000 per year with school-age children. One lives in a Comcast service area; the other lives three miles away in a neighborhood served only by AT&T. In 2012, the

first family gains access to \$9.95-per-month broadband through Internet Essentials, Comcast’s subsidized internet program for households below 185% of the federal poverty line. Both families face the same labor market, the same banks, the same credit products. But only one can now comparison-shop for a credit card after the kids go to bed, set up automatic bill payments from a computer, or apply for jobs online.¹

Internet Essentials increased internet adoption among eligible households by 3.7 percentage points, narrowing the digital divide by over a quarter. Employment and income rise, consistent with the broader broadband literature (Hjort and Poulsen, 2019; Dettling, 2017; Zuo, 2021). In credit markets, the effects span the full pathway from search to management: treated households submit more applications, open new accounts at higher rates, show sustained declines in delinquency, and collection agency inquiries fall. Savings participation also increases. However, equity market participation does not respond, and this null persists across financial literacy levels, employment conditions, and the income distribution.

The pattern across products is informative. Employment, savings, and credit are domains where these households already have the capacity to participate but face high costs of search and management that internet lowers. Equity investment is different: at household incomes near the poverty line, there is essentially no discretionary margin for investment (Vissing-Jørgensen, 2002; Campbell, 2006), and internet access cannot create the surplus income or institutional access that participation requires.

These findings relate to the work of Bogan (2008) and Hvide et al. (2024), who document positive effects of internet access on stock market participation. In both studies, the marginal household is plausibly near the equity market participation margin: Bogan (2008) studies the general US population, where baseline stock ownership exceeds 30%, and Hvide et al. (2024) studies Norway, where universal social insurance reduces the background risk that constrains investment. In our sample, baseline participation ranges from 3% to 7%. When we estimate an endogenous correlation between internet access and investment, we recover a

¹Among Internet Essentials customers, 63% report using their connection to pay bills and 52% say it helped someone in their household find a job (Comcast Corporation, 2016).

positive relationship similar in magnitude to Bogan (2008). This correlation disappears under our causal design, suggesting that the internet-investment link in observational data reflects joint selection into technology adoption and financial market participation (Malmendier and Nagel, 2011; Kuhnen and Miu, 2017; Das, Kuhnen, and Nagel, 2020). Our results establish a boundary condition: internet facilitates investment for populations near the participation margin but is insufficient for households facing structural constraints that information access alone cannot resolve.

Our study contributes to the household finance literature on participation costs (Campbell, 2006, 2016; Vissing-Jørgensen, 2002; Guiso and Sodini, 2013; Haliassos and Michaelides, 2003; Andersen, Campbell, Nielsen, and Ramadorai, 2020; Günsberg and Kuhnen, 2026). Information costs, background risk (Guiso, Jappelli, and Terlizzese, 1996; Benzoni, Collin-Dufresne, and Goldstein, 2007; Betermier, Jansson, Parlour, and Walden, 2012), financial literacy (Lusardi and Mitchell, 2014; Van Rooij, Lusardi, and Alessie, 2011), and behavioral frictions (Kuhnen and Knutson, 2011; Kuhnen and Miu, 2017; Das, Kuhnen, and Nagel, 2020) tend to vary together in the cross section, making it difficult to isolate the role of any single friction. Our setting provides plausibly exogenous variation in one barrier, information and transaction costs, while holding others fixed. Andersen et al. (2020) pursue a similar decomposition using Danish administrative data and a structural model; our approach uses a policy-induced shock and traces effects across products for US low-income households, for whom these barriers are most severe.

Our paper builds on a literature documenting that internet access reduces price dispersion and increases competition in household financial product markets, beginning with Brown and Goolsbee (2002) on life insurance. A more recent strand documents how fintech lenders have expanded credit access to traditionally underserved borrowers via online channels (Buchak et al., 2018; Fuster et al., 2019); we test whether broadband infrastructure itself — independent of specialized fintech lenders — produces similar effects on credit-market participation among low-income households. We also contribute to the broadband economics literature

(Hjort and Poulsen, 2019; Dettling, 2017; Zuo, 2021; Hvide, Meling, Mogstad, and Vestad, 2024). Most closely related is Kim (2024), who uses the same IE program to study mortgage refinancing. Our analysis differs in scope: we examine multiple dimensions of financial inclusion simultaneously, using both inquiry and tradeline data to trace the pathway from credit search through account openings to payment behavior. This combination of the application and performance margins, typically studied in isolation (Agarwal, Grigsby, Hortaçsu, Matvos, Seru, and Yao, 2024; Stango and Zinman, 2023; Gathergood, Mahoney, Stewart, and Weber, 2019), allows us to show that increased credit access is accompanied by improved account management rather than expanded risky borrowing.

2 Internet Essentials

In August 2011, Comcast launched Internet Essentials (IE), a subsidized broadband program for low-income families with school-age children, across its entire service area in 39 states and the District of Columbia.² The program was a condition of the Federal Communications Commission’s approval of Comcast’s merger with NBC Universal (FCC 11-4), which required Comcast to “commence a program . . . to substantially increase broadband adoption in low income homes throughout Comcast’s service area” within nine months of the transaction’s closing in January 2011. The program was timed to the back-to-school season, since eligibility verification runs through school districts.

Internet Essentials provides broadband service at \$9.95 per month, with the option to purchase a subsidized computer for less than \$150. At the time of launch, Comcast’s standard residential broadband plans ranged from approximately \$30 to \$50 per month, making the IE price a 70–80% discount off retail rates. The subsidized computer was priced at roughly one-third of the cheapest laptop available at retail, which in 2011–2012 started at \$350–\$450.³ These subsidies were meaningful for the target population: smartphones, while increasingly

²For a detailed description of the program, see case study 2 in Davidson, Santorelli, and Kamber (2012).

³The Samsung Chromebook, priced at \$249, did not launch until October 2012.

common, were owned by only 42% of adults earning below \$30,000 in early 2012, and mobile banking capabilities were limited to basic balance checks and transfers (Pew Research Center, 2012; Board of Governors of the Federal Reserve System, 2012).

Eligibility requires that the household include at least one child who qualifies for the National School Lunch Program (NSLP), which sets the income threshold at 185% of the Federal Poverty Line (FPL). A family of four, for example, qualifies with annual income below approximately \$46,000 in 2012.⁴ The household also cannot have had an active Comcast subscription within the previous 90 days, which ensures that the program serves households that were previously unconnected rather than existing subscribers seeking a discount.

Initial enrollment reached 150,000 by the end of 2012 and 750,000 families (3 million individuals) by August 2016 (Comcast Corporation, 2016). The typical IE household consists of four people, is headed by someone approximately 39 years old, and is disproportionately Hispanic (55%) and female-headed (74%). Before enrolling, 80% lacked a home internet subscription. Among enrolled households, 98% report using the internet for school work, 63% for paying bills, and 52% say it helped someone find a job.

These program characteristics are valuable for our research design. Because IE specifically targets households that lack broadband and cannot afford market-rate service, our estimates reflect the effect of internet access on a population that was previously digitally excluded. This contrasts with settings that exploit geographic broadband rollout (Hjort and Poulsen, 2019; Hvide, Meling, Mogstad, and Vestad, 2024), where the first adopters tend to be wealthier households that could already afford the service.

2.1 Identification

Our identification exploits three sources of variation generated by the IE program’s design. First, geographic variation arises from Comcast’s historical cable franchise territory, which was established decades before the IE program and is largely determined by local government

⁴Conditional on IE eligibility, average household income is approximately 100% of the FPL, well below the 185% threshold.

franchise agreements from the 1970s and 1980s. We measure Comcast’s market presence at the census block level using broadband provider data from the National Telecommunications and Information Administration (NTIA).⁵ We aggregate this binary indicator to the appropriate geographic level for each dataset (PUMA for the ACS, MSA for the CPS, zip code for Experian, and county for IRS) using population weights:

$$\text{Comcast Coverage}_i = \frac{\sum_{j=1}^J \text{Pop}_j \times \mathbf{1}\{\text{Comcast Present}\}_j}{\sum_{j=1}^J \text{Pop}_j} \quad (1)$$

Figure 1 displays Comcast coverage at the MSA level. Comcast operates primarily in urban and suburban markets, with substantial coverage variation across metropolitan areas.

Second, household eligibility varies cross-sectionally based on income and family composition, as described above. This generates within-location variation between eligible and ineligible households.

Third, the program’s launch provides time-series variation. Although IE began accepting applications in August 2011, enrollment was modest in the first year (approximately 41,000 households). Because the program scales with the school-year enrollment cycle and the first full calendar year of operation is 2012, we define the post-treatment period as 2012 through 2015.⁶ Our sample runs from 2009 to 2015. We end the sample in 2015 to avoid contamination from competing subsidized broadband programs that launched in 2016, including AT&T’s Access program and the expanded Lifeline program.

The combination of geographic, cross-sectional eligibility, and temporal variation forms the basis of our triple-difference design, which we describe formally in Section 4.

⁵See <https://www2.ntia.doc.gov/Jun-2012-datasets>.

⁶Our results are not sensitive to this choice. The 2011 base year in our event-time specifications is partially treated, which attenuates our post-treatment estimates and makes them conservative. We discuss this further in Section 6.

3 Data

We use four data sources to study the effects of internet access on financial inclusion: the American Community Survey (ACS), the Current Population Survey (CPS), Experian credit bureau records, and IRS Statistics of Income (SOI) tax filings. In both the ACS and CPS, we identify IE-eligible households as those with income below 185% of the FPL and at least one child between ages 5 and 17.

3.1 American Community Survey

We obtain ACS micro-level files from IPUMS for the period 2009 to 2015. The ACS provides individual-level data on employment, labor income, and investment income, with geographic identification at the PUMA level. Our primary sample consists of household heads below 200% of the FPL, yielding approximately 2.6 million observations. This threshold includes both IE-eligible households (below 185% with school-age children) and a natural control group of households that fall just above the eligibility threshold or lack school-age children.

Our main ACS outcome variables are employment status, labor income, and investment income. Following Cole, Paulson, and Shastry (2014) and Gan, Song, and Zhou (2022), we construct a binary indicator for investment participation equal to one if the household reports any nonzero INCINVST (including negative values, which indicate realized investment losses and therefore imply market participation).⁷ A limitation of INCINVST is that it combines capital gains/losses, interest, dividend, and rental income into a single variable. We cannot use it to distinguish between savings (interest) and equity market participation (capital gains/losses and dividends) in the ACS. We rely on the CPS for this distinction.

The ACS reports household internet access beginning in 2013, one year after IE launched.⁸ Because the internet access variable is unavailable during the pre-treatment period, we use

⁷Observations with INCINVST equal to -9999 are set to missing.

⁸Figure 2 plots the fraction of households with internet access by income level, showing the expected monotonic relationship. IE-eligible households fall in the lowest income bins, with substantially lower access rates.

the CPS to estimate the first-stage effect of IE on internet adoption.

3.2 Current Population Survey

We use CPS micro-level files to measure internet access, savings behavior, and dividend income. More than 70% of CPS observations can be located at the MSA level, which we match to MSA-level Comcast coverage. To estimate the effect of IE on internet adoption, we also construct repeated cross-sections spanning the pre- and post-treatment periods, by combining the CPS Educational Supplement (2007, 2009, 2010, 2012) and Computer and Internet Use Supplement (2011, 2013, 2015).

The CPS reports interest income and dividend income as separate variables. Interest income (INCINT) captures income from savings accounts, certificates of deposit, money market funds, bonds, and Treasury notes. Dividend income (INCDIVID) captures income from stocks and mutual funds. We construct binary participation indicators for each. The CPS sample is substantially smaller than the ACS, so the estimates are less precise, but the ability to separate savings from equity income is the primary advantage of the CPS for our analysis.

The CPS Annual Social and Economic Supplement (ASEC) is conducted each March, with income questions referencing the prior calendar year (Weinberg et al., 1999). A household surveyed in March 2013 reports income earned in calendar year 2012, the first year of IE availability.

3.3 Experian Credit Bureau Data

Credit is highly relevant for liquidity-constrained households. Because the ACS and CPS measure income flows rather than credit behavior, we turn to credit bureau records from Experian, which observe consumers' formal credit activity directly. We use two complementary types: inquiry data, which capture the application (search) margin, and tradeline data, which capture the accounts consumers hold and how they manage them.

3.3.1 Inquiry Data

The inquiry file records all hard inquiries (formal credit applications) submitted to Experian. A consumer initiates a hard inquiry by applying for credit, and the file identifies the type of institution making the inquiry through Experian’s Kind of Business (KOB) classification. We focus on inquiries across several credit product categories: credit cards, auto financing, mortgages, personal loans, and other bank credit. To measure financial distress, we separately identify inquiries from collection and recovery agencies, which typically follow missed payments or unresolved debts (Keys, Mahoney, and Yang, 2023; Barcellos and Jacobson, 2015; Hu, Kaestner, Mazumder, Miller, and Wong, 2018).

The inquiry file contains only hard inquiries, not soft inquiries (such as preapproval screenings or lender-initiated market surveys). This distinction matters because it means that the inquiry data we employ capture active credit-seeking behavior rather than lender marketing.

3.3.2 Tradeline Data

The tradeline file records all credit accounts associated with each individual, including the account type, opening date, current balance, credit limit, payment status, and delinquency history. While the inquiry data capture the application margin, the tradeline data reveal whether applications result in actual accounts and how borrowers manage those accounts over time.

We process the full tradeline files for 2009 through 2015, which contain approximately 35 to 40 million records per year. For each individual-year, we construct several categories of outcome variables. On the extensive margin, we measure whether the individual holds an open account of each major type (credit card, auto loan, mortgage, personal loan, collection account). On the new-account margin, we identify accounts opened within the current year based on the account opening date. On the credit management margin, we use the 30-day, 60-day, and 90-day delinquency counters attached to each tradeline, along with the aggregate

derogatory mark indicator. We also observe the individual’s VantageScore (credit score) in each year from the associated header file.

3.3.3 Eligibility Proxies

A limitation of the Experian data is that they do not include demographic characteristics such as age, household income, or the presence of school-age children. We therefore cannot directly identify IE-eligible individuals. We address this using two proxy measures.

Our primary proxy uses individual-level pre-treatment credit scores. We calculate the average VantageScore for each individual during the pre-treatment period (2009 to 2011) and classify individuals with scores below 600 as likely IE-eligible. Low credit scores are strongly correlated with low income in this population, and the pre-treatment measurement ensures the proxy is not affected by the treatment itself.

Our secondary proxy uses zip code level median household income measured in 2011, classifying zip codes with median income below \$30,000 as low-income. This proxy captures geographic disadvantage but cannot distinguish higher-income individuals living in low-income areas. Under either proxy, measurement error is likely to attenuate our estimates by including ineligible individuals in the treated group.

3.4 IRS Statistics of Income

We obtain county-level tax filing data from the IRS Statistics of Income (SOI), which have been widely used to measure household financial outcomes (Brown, Ivković, Smith, and Weisbenner, 2008; Lin, 2020; Clifford, Gerken, and Qiu, 2023; Farizo, Gerken, and Wu, 2023). The SOI data report the number of tax returns claiming various income types by county and adjusted gross income (AGI) bracket, allowing us to measure employment (returns with W-2 income and EITC), savings (returns with 1099-INT interest income), and equity participation (returns with 1099-DIV dividend income) among low-income filers.

Because the county-level SOI data for 2009 do not include AGI bracket information

necessary for our identification, the IRS sample runs from 2010 to 2015.⁹ The IRS data provide two advantages relative to the Census data. First, they cover all US counties, including less-populated areas that are underrepresented in the ACS and CPS. Second, misreporting tax information carries legal consequences, whereas survey responses do not. The IRS results therefore serve as a useful robustness check on our Census-based estimates.

3.5 Summary Statistics

Table 1 reports summary statistics from the ACS for our primary sample of household heads below 200% of the FPL. Panel A reports the full sample; Panel B restricts to IE-eligible households. Relative to the full low-income sample, IE-eligible households have lower income (mechanically, given the 185% threshold) and more children (mechanically, given the school-age-child requirement). They are also more likely to be Black or Hispanic and have approximately 1.5 fewer years of education. Despite the requirement for school-age children, the marriage rate among IE-eligible households is not substantially higher than in the full sample, reflecting a significant fraction of single-parent households.

Baseline characteristics from the Experian data illustrate the credit profile of the eligible population. Among individuals with pre-treatment VantageScores below 600 in 2011, the average score is 620, and 73% have at least one collection account on their credit file. Approximately 37% hold a bank credit card, 10% have an auto loan, and 4% have a mortgage. Fewer than 0.1% hold accounts with investment firms. These baseline rates underscore both the active engagement of this population with credit markets and their near-complete absence from investment markets.

⁹See <https://www.irs.gov/statistics/soi-tax-stats-county-data>.

4 Empirical Strategy

We first verify that IE increased internet access among eligible households using a difference-in-differences specification within the IE-eligible population:

$$\text{Internet}_{h,l,t} = \alpha_1 \cdot (\% \text{Provider}_l \times \text{Post}_t) + \mathbf{X}'_{h,t} \boldsymbol{\delta} + \gamma_t + \gamma_l + \epsilon_{h,l,t} \quad (2)$$

We estimate this equation separately for each major internet provider (Comcast, Verizon, AT&T, Charter) and jointly in a horserace specification. If the IE program is driving the increase in internet access, we expect a positive coefficient only for Comcast. We present the first-stage effect on internet adoption, and what it implies for interpreting magnitudes, in Section 5.1.

We then exploit the three sources of variation described in Section 2 to estimate the causal effect of internet access on financial outcomes. Our baseline specification for the ACS and CPS is a triple-difference model:

$$\begin{aligned} Y_{h,l,t} = & \beta_1 \cdot (\% \text{Comcast}_l \times \text{Post}_t \times \text{Eligible}_h) + \mathbf{X}'_{h,t} \boldsymbol{\delta} \\ & + \gamma_{\text{Eligible} \times t} + \gamma_{\text{Eligible} \times l} + \gamma_{l \times t} + \epsilon_{h,l,t} \end{aligned} \quad (3)$$

where h indexes a household, l denotes geographic location (PUMA in the ACS, MSA in the CPS), and t is the year. $\% \text{Comcast}_l$ is the population-weighted share of census blocks in location l served by Comcast. Post_t equals one for $t \geq 2012$. Eligible_h equals one if the household falls below 185% of the FPL and includes at least one school-age child. The coefficient of interest, β_1 , captures the differential change in outcome Y for IE-eligible households in areas with greater Comcast coverage after the program's launch. The three control groups are ineligible households in Comcast areas, eligible households outside Comcast areas, and ineligible households outside Comcast areas.

The vector $\mathbf{X}_{h,t}$ includes controls for the household head's gender, age, age squared, race, Hispanic origin, marital status, years of education, and number of children. The

fixed effects absorb eligibility-by-year trends ($\gamma_{\text{Eligible} \times t}$), eligibility-by-location time-invariant differences ($\gamma_{\text{Eligible} \times l}$), and location-by-year shocks ($\gamma_{l \times t}$). Standard errors are clustered at the geographic level (PUMA or MSA).

Because multiple members of a household may share financial accounts, we measure financial outcomes at the household level: if any individual within the household reports investment income, interest income, or dividend income, we classify the household as participating.

The Experian credit bureau data do not include household demographics, so we cannot directly identify IE-eligible individuals. We instead use two proxy measures for eligibility, as described in Section 3. The Experian specifications follow the same triple-difference structure as equation (3), with the individual’s pre-treatment credit score or zip code median income replacing the direct eligibility measure, and individual and zip code fixed effects replacing the household-level fixed effects. Standard errors are clustered at the zip code level.

The triple-difference design requires that, absent IE, the difference in outcomes between eligible and ineligible households would have evolved similarly in Comcast and non-Comcast areas. This assumption is weaker than the parallel trends assumption required by a standard difference-in-differences, because the triple interaction differences out any location-specific or eligibility-specific trends. We assess the plausibility of this assumption using event-time specifications that replace Post_t with a full set of year indicators (omitting 2011 as the base year) and report the resulting coefficients graphically. At the sub-MSA level, Comcast’s coverage reflects municipal franchise agreements established in the 1970s and 1980s, well before the internet era, and is therefore predetermined with respect to the treatment.

Throughout, we report intent-to-treat (ITT) effects of IE program eligibility rather than treatment-on-the-treated effects of internet access, since the ITT measures the policy-relevant effect of making subsidized broadband available.

5 Results

We begin with a simple premise: the effects of internet access depend on which financial margins are relevant for low-income households. Internet access reduces information and transaction costs, but these reductions matter only when they interact with an economic decision the household is otherwise positioned to make. For low-income households, the central financial problem is not portfolio optimization. It is managing liquidity, payment obligations, and financial shocks with limited cash on hand.

Following the household finance literature on participation costs and inaction (Andersen et al., 2020; Vissing-Jørgensen, 2002; Gomes et al., 2021), we distinguish barriers to financial participation along three observable dimensions: the intensity of search required to compare offerings across providers, the stringency of qualification gates that exclude households independent of information (credit-score thresholds, collateral requirements, income tests), and the fixed cost of participation (account minimums, broker minimums, discretionary-income requirements). A barrier is *frictional* when search and information are the binding constraint; it is *structural* when wealth, qualification, or background risk is binding. Internet access addresses the first; it does not address the second. Our distinction echoes Peress (2005)’s decomposition of stock-market participation into information costs and entry costs, with the former responsive to technology and the latter tied to wealth and qualification.

Applied to our outcomes, this distinction generates a hierarchy of predicted responses. Credit markets satisfy both conditions. Credit is highly relevant for financially constrained households because it can relax short-run liquidity constraints, smooth consumption, finance durable purchases, and help manage income shocks. Credit-market participation involves substantial search, application, monitoring, and payment costs that internet access can plausibly reduce. We predict positive effects on credit search, account openings, and credit management.

Savings is an intermediate case. Precautionary liquidity is valuable for low-income households, and internet access can reduce banking frictions by making it easier to monitor bal-

ances, transfer funds, and manage accounts remotely. However, savings requires surplus income that internet access does not directly create. We predict a positive but smaller and less robust response than the credit response.

Risky investment is the limiting case. Equity-market participation requires discretionary wealth, tolerance for risk, and sufficient precautionary buffers. The binding constraint is structural rather than informational; even with better information, households with little liquid wealth and high background risk have strong reasons to prioritize bill payment, debt reduction, and emergency funds over risky assets. We predict no meaningful effect on equity participation.

The framework is falsifiable along three margins. First, a positive equity response would indicate that information frictions, not structural constraints, govern equity participation in this population. Second, a null effect on credit search would indicate that information frictions are not binding even in the activity most closely tied to households' liquidity needs. Third, an inverted hierarchy — with the investment response exceeding the credit response — would contradict the framework's central premise that internet access can only relax frictional barriers, not structural ones.

These predictions organize the empirical analysis. We test whether internet access affects financial outcomes along a hierarchy of household needs: credit access and credit management, savings, and risky investment. The framework predicts the strongest effects in credit markets, more limited effects on savings, and no meaningful effect on risky investment. The comparison across outcomes is therefore central to the paper. It allows us to distinguish financial activities where digital frictions are binding from activities where deeper structural constraints remain binding even after internet access improves.

5.1 Internet Access

We first verify that IE achieved its primary objective of increasing internet access among eligible households. Table 2 reports estimates of equation (2) using CPS microdata. Col-

umn 1 shows that Comcast coverage is associated with a 5.3 percentage point increase in internet access for each unit of Comcast coverage among eligible households after the program’s launch ($\hat{\beta} = 0.053$). Evaluated at mean Comcast coverage (70%), this corresponds to a 3.7 percentage point ($= 5.3 \times 70\%$) increase in internet access. To put this magnitude in context, 75.2% of IE-eligible households reported internet access at home, compared to 88.9% of ineligible households, implying a digital divide of 13.7 percentage points. The IE program therefore closed over a quarter of this gap.

Columns 2 through 4 report placebo tests using coverage by Verizon, AT&T, and Charter. None of the other providers show a significant post-2012 effect on internet access, and Column 5 includes all four providers simultaneously: despite multicollinearity from overlapping service areas, the Comcast coefficient remains similar in magnitude and statistical significance. This specificity matters because Comcast operates primarily in eastern urban markets, including the five largest IE cities—Chicago, Houston, Miami, Philadelphia, and Fresno. The placebo tests indicate that the increase in access is driven by the IE program rather than by characteristics of Comcast’s service territory or by broader trends in broadband adoption.

The program’s active enrollment process shapes who benefits. Comparing eligible households in Comcast areas with and without internet access, adopters are more likely to be male, older, married, and more educated, and less likely to be Black or Hispanic. The differential adoption rate between Comcast and non-Comcast areas reinforces this: the Comcast premium is concentrated among men (+7.5 percentage points versus -1.0 for women), married households (+5.3 versus -1.1), and those with at least some college (+1.5 versus -4.7 for high school or less). These patterns are consistent with an enrollment process that requires program awareness and engagement with application procedures, and imply that our ITT estimates may understate the effects of policies that reach a broader set of eligible households.

The modest first stage also shapes how we interpret magnitudes. It implies a scaling

factor of roughly 27 from the ITT to a treatment-on-the-treated effect of internet access.¹⁰ We report ITT estimates throughout because they measure the policy-relevant effect of making subsidized broadband available. Scaling instead to a TOT yields implausibly large magnitudes—for example, scaling the 3 percentage point savings effect by the first stage implies a TOT exceeding the baseline participation rate—suggesting that IE eligibility generates benefits beyond direct internet access, including digital literacy programming, community awareness effects, and subsidized computer access. These bundled channels would not appear in the internet-access first stage but could contribute to the reduced-form effects. The bundled nature of the treatment also strengthens the investment null: if the full IE package produces no investment response, it is unlikely that internet access alone would do so.

5.2 Employment and Income

Table 3 reports estimates of the triple-difference specification in equation (3) using ACS data, with employment status and labor income as outcomes. Our baseline sample includes all household heads below 200% of the FPL, which provides two natural control groups: households above 185% FPL (income-ineligible) and households below 185% FPL without school-age children (composition-ineligible).

Columns 1 and 2 show that IE eligibility is associated with a 0.8 percentage point increase in employment and a \$142 increase in annual labor income. For context, Qiu (2025) finds that a 0.8% increase in local government spending produces a \$73 increase in labor income for comparable households using the same ACS microdata. The IE effect on income is therefore comparable to approximately a 1.5% increase in local government spending.

Columns 3 and 4 restrict the sample to households below 150% FPL, where all obser-

¹⁰The first-stage F -statistic of 5.8 falls below the conventional threshold of 10 (Staiger and Stock, 1997), though it exceeds the critical values for maximal size distortion with a single endogenous variable. The small first stage reflects the nature of the program: IE reaches a targeted subset of eligible households through an active enrollment process, and many eligible households do not enroll because they are unaware of the program, choose not to participate, or gain internet access through other means.

vations are income-eligible and the identification relies solely on the presence of school-age children. The effects strengthen modestly, consistent with the treatment being more relevant for lower-income households. Columns 5 through 8 expand the sample to 400% FPL and the full population, respectively. As expected, the effects attenuate as the sample includes higher-income households for whom IE is not relevant.

Event-time estimates in Figure 3 show no significant pre-trends in either outcome. The employment and income effects emerge in the first post-treatment year and grow through 2015 as enrollment accumulates. In Appendix Table IA.1, we also confirm increases in filings with W-2 and EITC, further corroborating the causal impact of internet access on improved labor market outcomes.

5.3 Credit Markets

We examine the effect of internet access on credit markets using Experian data. Unlike the Census-based outcomes, the Experian data allow us to observe both the search margin (credit applications) and the outcome margin (actual accounts and payment behavior). We organize the credit results into three categories: credit search, credit acquisition, and credit management.

5.3.1 Credit Search

We begin with credit search, where lower information and comparison costs should appear first. Credit inquiries provide a direct measure of the search and application margin because they reflect households' active credit applications rather than lender-initiated marketing. Table 4 reports the effect of IE on credit inquiries across product categories. Panel A measures the number of inquiries an individual submits in a year; Panel B measures whether the individual submits any inquiry at all. Panel A shows that treated households submit 5.3% more credit card inquiries relative to the baseline rate, and that collection agency inquiries fall by 8.6%. Auto loan inquiries increase by 4.4% and mortgage inquiries by

41.9%; the mortgage baseline is 0.011 inquiries per person-year, so the large proportional effect corresponds to an absolute increase of 0.005 inquiries.¹¹ Personal loan inquiries are positive in both panels but are not statistically significant in either.

Comparing the two panels separates households entering credit search for the first time from households already searching who now search more. Because the expected number of inquiries is the probability of any inquiry multiplied by the number of inquiries conditional on searching, and eligible households average 1.77 credit card inquiries per searcher at baseline, the increase in search participation in Panel B accounts for roughly 23% of the increase in inquiry volume in Panel A. The remaining three quarters reflects households already in the credit market applying more often. Collections show the opposite composition: roughly two thirds of the decline reflects households leaving collections entirely rather than accumulating fewer collection inquiries. Internet access therefore operates on both margins, but in different proportions across products—mostly intensified comparison shopping for credit cards, and mostly exit for collections.

Two limits on this decomposition are worth stating. It treats the triple-difference coefficients as marginal effects evaluated at the baseline and omits the interaction term, so the shares are approximations rather than estimates. More importantly, because the specification includes individual fixed effects, it identifies movement only among households already present in the credit file. Households that enter the credit bureau data for the first time during the sample period contribute nothing to the estimate, so the extensive-margin share should be read as a lower bound on genuinely new entry into credit search.

The final column collects other bank credit inquiries (KOB BB) — bank-group inquiries

¹¹Because Panel A counts inquiries, the auto and mortgage columns are exposed to rate shopping: a household comparing lenders for a single loan can generate several inquiries in a short window. Credit bureaus treat auto, mortgage, and student inquiries falling inside a 14- to 45-day window as one event when computing scores, but the inquiry file records each application separately. The auto and mortgage counts in Panel A may therefore overstate the number of distinct credit applications. Two considerations bound the concern. Credit cards, the product behind our headline result, are not subject to these de-duplication windows, so the card estimate is unaffected. And Panel B, which measures only whether a household applies at all, is immune to rate shopping in every column; the auto and mortgage effects remain positive and significant there.

that are neither credit cards nor mortgages, both of which have their own columns — and shows no significant response in either panel. We report it for completeness rather than as a test of banking access. The inquiry file records only hard credit inquiries, so opening a checking or savings account generates no observation in these data, and no column of this table measures deposit-account use. For the products that require comparison across lenders—credit cards, auto loans, and mortgages—the reduction in search costs is substantial.

Event-time estimates in Figure 4, which track the participation margin in Panel B, show that the credit inquiry effects emerge in the first post-treatment year and persist through 2015, with no evidence of differential pre-trends.

5.3.2 Credit Acquisition

Increased search activity does not necessarily translate into new accounts. Households may apply for credit and be rejected. Distinguishing between inquiries and account openings is therefore important for interpreting welfare. More inquiries alone could reflect wasted effort or unsuccessful applications, whereas new account openings indicate that lower search frictions translate into actual access to credit. To test whether internet access affects actual credit holdings, we use tradeline data from the full Experian tradeline files.

Table 5 reports the effect of IE on new account openings by product type. Treated households open new credit card accounts at a 10% higher rate relative to the baseline ($= 0.0047/0.046$). New auto loan accounts increase by 6% ($= 0.0011/0.018$), and new mortgage originations increase significantly from a very low base. The pattern across products is broadly consistent with the inquiry results: the products with the largest increases in search activity also show the largest increases in actual account openings. The magnitude and concentration of these account-opening effects are consistent with the fintech-lending literature, which finds that digital channels reduce processing time and expand credit to borrowers historically underserved by traditional banks (Fuster et al., 2019; Buchak et al.,

2018).

We note that the tradeline event-time estimates for new credit card and auto accounts show non-zero pre-treatment coefficients (Figure 5), with treated households below their 2011 level in both 2009 and 2010. The coefficients are small and do not follow a monotone path, so they are better read as year-to-year variation than as a trajectory; the differential impact of the post-financial-crisis credit contraction on subprime borrowers in Comcast versus non-Comcast areas is a plausible source. The treatment effects emerge after 2012 and grow steadily through 2015. We examine what these pre-treatment coefficients do and do not imply in Section 6.

5.3.3 Credit Management

For low-income households, the key question is not just whether internet access helps them obtain more credit, but whether it also helps them manage existing obligations more effectively. Internet access makes it easier to monitor balances, track due dates, communicate with servicers, and set up automatic payments, thereby reducing repayment frictions. We therefore examine whether treated households become less likely to fall delinquent on their existing accounts. Table 6 shows that they do. The probability of any 30-day late payment falls by 0.6 percentage points relative to a baseline of 47.5%—a reduction of roughly 1.2%—and the effects are similar for 60-day and 90-day delinquencies.

Figure 6 presents the event-time estimates for 30-day delinquency. The pre-treatment coefficients in 2009 and 2010 are close to zero and statistically insignificant, indicating clean parallel pre-trends. After IE launches, delinquency declines monotonically in each year, reaching a reduction of approximately 1 percentage point by 2015. This is the cleanest event-time pattern among all tradeline outcomes and provides strong evidence that internet access generates real improvements in how households manage their existing credit obligations.

The delinquency decline likely reflects both direct channels (automatic payments, balance monitoring, due-date tracking) and indirect effects through the employment and income gains

documented in Section 5.2.

Credit scores reflect these dynamics. VantageScore shows a temporary dip in 2012 from new hard inquiries, followed by recovery to baseline by 2015 (Figure 7). The probability of subprime status (score below 600) declines steadily, reaching significance by 2014–2015 (Figure 8). The initial score penalty from increased applications is offset over time by fewer missed payments.

The combination of increased credit access with reduced delinquency indicates that internet is not simply expanding risky borrowing; treated households engage with credit markets on better terms.

5.4 Savings

Savings products involve lower informational barriers than equity investment, and internet access may reduce transaction costs by making it easier to monitor balances, transfer funds, and use online banking tools. We therefore test whether treated households become more likely to report interest income. Table 7 reports the effect of IE on savings participation using CPS data. The outcome variable is a binary indicator for whether the household reports any interest income, which captures participation in savings accounts, certificates of deposit, and similar interest-bearing instruments.

Column 1 shows a 3 percentage point increase on a base of 20%, in line with Xue et al. (2011). Among IE customers, 63% report using their connection to pay bills (Comcast Corporation, 2016), suggesting that online banking is among the most immediate financial applications of broadband.

This result is sensitive to the sample period. When we exclude the years 2009 and 2010 from the estimation, the savings coefficient becomes statistically insignificant (Appendix Table IA.2). This sensitivity reflects the fact that the pre-treatment period includes years of significant macroeconomic disruption that may differentially affect savings behavior across treatment and control groups. Event-time estimates in Figure 9 show that the savings

participation response emerges in 2012–2013 but is imprecisely estimated, consistent with the pre-trend sensitivity documented in Appendix Table IA.2. The IRS SOI data provide independent support: the fraction of low-income tax returns reporting interest income (1099-INT) increases significantly in Comcast counties after 2012 (Appendix Table IA.1). The ACS, which combines interest, dividend, and rental income into a single variable, does not show a significant effect on this broader measure (Appendix Table IA.3). We interpret the savings evidence as positive but measured with limited precision: two of three data sources point in the same direction, but the CPS result is sensitive to the sample period and should be interpreted with appropriate caution.

Appendix Table IA.4 asks whether the savings effect is concentrated where barriers to banking are most severe. We split the sample four ways: by state financial literacy (FINRA test scores), by the state share of unbanked households (FDIC survey), and by two pre-treatment features of the local banking market, deposit concentration and distance to the nearest branch. The estimate is positive in all eight subsamples and roughly twice as large on the more constrained side of every split. The differences are not statistically significant, however; the smallest p -value for equality within a pair is 0.45. The pattern is consistent with internet access mattering most where banking frictions are greatest, but it does not statistically establish that gradient.

Two features of the table limit the weight it can bear. First, the literacy and unbanked splits are close to the same partition: 22 of the 24 low-literacy states are also high-unbanked states, and 91% of households receive the same classification under both. Columns 1 through 4 are best read as a single test rather than two. Second, those subsamples are unbalanced, so the high-literacy and low-unbanked estimates carry standard errors near 0.04 and could not have detected an effect of the magnitude we estimate elsewhere. The bank market structure splits in columns 5 through 8 are balanced and more precisely estimated, and there the coefficient roughly doubles as deposit concentration rises and as branches become more distant, though equality still cannot be rejected.

5.5 Investment Participation

We next examine investment participation, which provides the most demanding test of our friction-based framework. Unlike credit search or basic savings, equity market participation requires households not only to access information but also to have sufficient disposable resources and willingness to bear financial risk. Internet access may reduce information frictions, but it does not remove the fixed costs, background risk, or income constraints that are especially relevant for low-income households.

We begin with a motivating contrast. In OLS regressions controlling for income, demographics, and PUMA-by-year fixed effects, internet access is associated with a 3.2 percentage point higher investment rate (Appendix Table IA.5), consistent with Bogan (2008). This correlation disappears entirely under the causal design: Table 8 reports the causal effect of IE on household investment participation. Columns 1 and 2 use ACS data, with the outcome equal to one if the household reports any nonzero investment income. The coefficient is small, negative, and statistically insignificant. Columns 3 and 4 use CPS dividend income as a cleaner measure of equity market participation and confirm the null result.

Event-time estimates in Figure 10 show no evidence of an investment response in any post-treatment year. In the ACS (panel a), the pre- and post-treatment coefficients are all economically and statistically indistinguishable from zero, with tight confidence intervals reflecting the large sample size. The CPS estimates (panel b) are noisier but also centered on zero throughout the sample period.

5.5.1 Bounding the Null

The null result is not an artifact of low statistical power. With approximately 2.6 million ACS observations, the standard error on the triple-difference coefficient is 0.002, implying a minimum detectable effect (at 80% power and 5% significance) of approximately 0.5 percentage points, or 16% of the baseline investment rate. Using a two-one-sided-tests (TOST) equivalence procedure, we can reject at the 5% level that internet access increases invest-

ment participation by more than 20% of the baseline rate. The CPS sample is substantially smaller and cannot bound the null as tightly.

5.5.2 Cross-Sectional Tests

Several explanations could account for the investment null: insufficient financial literacy, insufficient income stability, or structural constraints unrelated to information frictions. We test whether the null varies along dimensions that distinguish among them.

Appendix Table IA.6 reports investment effects separately by financial literacy and pre-treatment employment conditions. Columns 1 and 2 split the CPS sample by state-level financial literacy, measured by FINRA test scores. If cognitive complexity were the binding constraint, internet access should increase investment in high-literacy areas where households have the knowledge to act on improved information. We find null effects in both high- and low-literacy states, with a p -value of 0.586 for the difference between groups.

Columns 3 and 4 split the ACS sample by pre-treatment PUMA-level employment rates. If background income risk were the binding constraint, internet access should increase investment in areas with higher baseline employment, where income stability makes investment more attractive. We find null effects in both subgroups, with a p -value of 0.611 for the difference.

Appendix Table IA.7 tests whether the null varies by income within the eligible population by interacting the triple-difference with indicators for household poverty level. The investment null is uniform across the income distribution: households near the poverty line, at 150% of the poverty line, and near the 185% eligibility threshold all show null effects on investment participation.

The cross-sectional tests indicate that the investment null is not explained by financial literacy, employment conditions, or income variation within the eligible population.

The Experian data reinforce this interpretation. Fewer than 0.1% of individuals hold accounts with investment firms, and fewer than one in three thousand credit inquiries originates

from an investment firm. This is not a population deterred from investing by high search costs; it is a population for which investment is not a relevant financial activity. Multiple structural constraints likely bind simultaneously: insufficient discretionary income (Vissing-Jørgensen, 2002), minimum account balance requirements, and trust and beliefs shaped by socioeconomic experience (Guiso, Sapienza, and Zingales, 2008; Kuhnen and Miu, 2017; Das, Kuhnen, and Nagel, 2020).

Our null result is consistent with the prior literature once population differences are accounted for. Bogan (2008) and Hvide et al. (2024) find positive effects on investment in populations where baseline participation exceeds 30% or where strong social insurance reduces background risk. In our sample, structural constraints dominate: insufficient discretionary income, limited institutional access, and near-zero baseline participation make information frictions irrelevant to the investment decision. The positive endogenous correlation in our data (presented previously in Appendix Table IA.5) that disappears under the causal design is consistent with the internet-investment link reflecting selection rather than causation.

We note that the frictional-versus-structural distinction makes its sharpest predictions at the extremes. At one pole, credit is a frictional domain, and the response is large and consistent from search through acquisition to management. At the other, equity participation is structural, and the null is tight enough to reject effects larger than 20% of the baseline rate. Savings sits between the poles: it involves moderate barriers of both types, so the framework predicts a moderate effect, and the evidence is consistent but not a particularly discriminating test.

6 Robustness

6.1 Pre-Trend Sensitivity

Several tradeline outcomes exhibit non-zero pre-treatment coefficients in the event-time specifications. We take the event-time estimates as the primary evidence on pre-trends, assess each outcome in turn, and then report a linear trend control as a secondary check.

The most direct test is whether a differential trend, fitted to the pre-treatment period and continued forward, can account for what we observe after 2012. For each outcome we fit a line through the three pre-treatment event-time coefficients and extrapolate it across the post-treatment window. If the results reflected a pre-existing trajectory rather than the program, that extrapolation should reproduce the post-treatment path.

For 30-day delinquency the pre-treatment coefficients are 0.0008 in 2009 and 0.0014 in 2010, roughly 0.2% and 0.3% of the 47.5% baseline, and the fitted pre-treatment slope is -0.0004 per year. Extended to 2015 it predicts a differential of -0.0016 , against an observed -0.0104 (Figure 6). A linear continuation of the pre-treatment path accounts for 15% of the 2015 effect; the remaining 85% cannot be produced by any differential trend consistent with what we see before treatment. For new credit card openings the pre-treatment coefficients are -0.0017 in 2009 and -0.0033 in 2010, the fitted slope is 0.00085 per year, and extrapolation predicts 0.0034 in 2015 against an observed 0.0083 (Figure 5). The pre-treatment trajectory accounts for 41% of the 2015 effect and cannot generate the remainder. The pre-treatment path for new accounts is in any case not a trend. Relative to the 2011 base year the series runs -0.0017 , -0.0033 , 0 , then -0.0002 , 0.0012 , 0.0053 , and 0.0083 from 2012 through 2015: it falls from 2009 to 2010 and then rises into the base year, so there is no monotone trajectory to project.

The linear trend control in Table IA.8 tells the same story. It adds the interaction of treatment status with a linear year variable centered at 2011, so the implied treatment effect at horizon h is the level coefficient plus h times the trend coefficient; the level coefficient

on its own measures the differential at 2011, a pre-treatment year, and is not the treatment effect. Read this way, the trend-augmented specification traces almost exactly the path the unrestricted event study traces. For new credit card openings it implies -0.0002 , 0.0021 , 0.0044 , and 0.0067 from 2012 through 2015, against event-time estimates of -0.0002 , 0.0012 , 0.0053 , and 0.0083 . For delinquency it implies -0.0017 , -0.0037 , -0.0057 , and -0.0077 , against -0.0023 , -0.0032 , -0.0066 , and -0.0104 . Adding the control does not overturn the results; it redescribes them.

By the final year of the sample the trend-augmented estimates are larger than the pooled ones, not smaller: 0.0067 for new credit card openings against a pooled 0.0047 , and -0.0077 for delinquency against a pooled -0.0059 . The pooled estimate averages across a window that begins in 2012, when roughly 41,000 households had enrolled, so it understates the effect once take-up accumulates. Any impression that the trend control attenuates these results comes from comparing its 2011 intercept with a post-period average rather than comparing like with like.

Two further points support this reading. The estimated trend coefficient for new credit card openings, 0.0023 per year, sits between the pre-treatment slope of 0.00085 and the post-treatment slope of 0.00296 , and roughly two thirds of the way toward the latter — so the control is identified largely from post-treatment variation and absorbs the growth of the treatment effect rather than a pre-existing trajectory. And with only three pre-treatment years, two of which coincide with the post-crisis recovery, a linear trend control is weakly identified to begin with: fitting a trend through three points dominated by recession dynamics risks overfitting the pre-period and absorbing genuine treatment effects (Roth, 2022). We report the exercise in Table IA.8 for completeness, but the event-time estimates and the extrapolation exercise above are the more informative evidence.

Collections are the one outcome with a genuine pre-treatment trajectory. Dropping the 2009 and 2010 observations reduces the estimated effect on collection inquiries from -0.011 to -0.006 , an attenuation of roughly 45%, though the estimate remains significant at the

1% level (Appendix Table IA.9). We treat the collections decline as attenuated but robust.

We summarize the robustness of each credit result as follows. The delinquency decline is the most robust: pre-treatment coefficients are close to zero, the post-treatment path is monotonic, and a linear continuation of the pre-treatment trajectory accounts for only 15% of the 2015 effect. The new account opening results have small and non-monotone pre-treatment coefficients, and the trend-augmented specification reproduces the event-time path rather than overturning it. The collections decline is the one result that attenuates materially, falling by roughly 45% when the earliest pre-treatment years are dropped, though it remains statistically and economically significant.

6.2 Multiple Testing Corrections

We test effects on multiple outcomes, raising the concern that some significant results may reflect chance. To address this concern, we apply the Benjamini-Hochberg false discovery rate (FDR) procedure to the primary results across all outcome categories. Appendix Table IA.10 reports the raw and adjusted p -values. Nine of fourteen hypotheses clear the 5% threshold and eleven clear the 10% threshold, spanning the internet-access first stage, the labor market outcomes, the credit results, and savings participation. As expected, the three investment outcomes—which are null in the baseline specification—do not survive, reinforcing that the equity result reflects a genuine absence of effect rather than a marginally significant one.

6.3 Varying the Income Cutoff

Our baseline sample includes household heads below 200% of the FPL. Appendix Table IA.11 reports results using alternative thresholds: 150%, 400%, and the full sample. The credit and investment results are robust across cutoffs. Consistent with the treatment being most relevant for lower-income households, the effects are somewhat stronger in the more restrictive samples and attenuate as higher-income households are included.

6.4 Eligibility Proxy Sensitivity

We present Experian results using the pre-treatment credit score proxy (VantageScore below 600) in the main text and the zip-level income proxy (median income below \$30,000) in Appendix Tables IA.12 and IA.13. Appendix Table IA.13 repeats the inquiry specifications of Table 4 under the income proxy; Appendix Table IA.12 reports account holdings, a stock measure, for which we emphasize the flow analogues (new accounts, delinquency) in the main text. Estimated inquiry effects are larger in magnitude under the income proxy in every column except the residual bank-credit category, where neither estimate is distinguishable from zero. The two proxies do not identify the same households: the credit score proxy selects individuals who are credit-constrained as well as likely low-income, and those individuals already submit substantially more inquiries at baseline in every category. We therefore read the two sets of estimates as describing partially overlapping populations rather than as a robustness check on one another.

Using ACS data to gauge proxy accuracy, approximately 15.8% of households in low-income PUMAs meet full IE eligibility criteria, implying substantial attenuation from measurement error. We do not scale our estimates given the additional assumptions this would require.

Appendix Table IA.14 varies the credit score threshold (500, 550, 650, 700). Credit card inquiries show the largest effect at the most restrictive threshold (< 500 , coefficient = 0.010, $p < 0.01$) and attenuate at higher thresholds, consistent with effects concentrated among the most credit-constrained borrowers.

6.5 Event-Time Base Year

Our event-time specifications omit 2011 as the base year. Because IE launched in August 2011, the base year is partially treated: households that enrolled in fall 2011 had several months of internet access before the end of the calendar year. This partial treatment in the base year has two implications. First, all post-treatment event-time coefficients are

measured relative to a partially treated baseline, which attenuates the estimated effects. The true treatment effects are therefore at least as large as those we report, and likely somewhat larger.

Second, partial treatment of the base year can generate apparent pre-trend artifacts. If treated households experience a small positive shift in 2011 (the base year), then earlier years will appear to have lower outcomes by comparison. This is consistent with several patterns in our event-time figures: the 2010 VantageScore spike (measured relative to a 2011 in which scores may have already begun improving) and the slow onset of credit inquiry effects. These patterns are mechanically consistent with comparing earlier years to a partially treated base rather than reflecting genuine pre-treatment divergence. All of our estimates should therefore be interpreted as conservative.

7 Conclusion

We use Comcast’s Internet Essentials program to study how internet access affects financial inclusion among low-income US households. The program provides a clean setting for this question: it targets households that were previously unconnected, it generates three sources of identifying variation, and it operates during a period before competing programs entered the market.

Internet access improves financial outcomes where frictional barriers bind. Employment and income rise. In credit markets, the effects span from search through acquisition to management, and the combination of increased access with reduced delinquency indicates that treated households engage with credit on better terms. But internet access does not affect equity market participation. The null is precisely estimated, robust across four data sources, and uniform across subsamples defined by financial literacy, employment conditions, and income. This establishes a clear boundary on what technology-based interventions can achieve for financial inclusion among low-income households.

These findings have direct implications for policy. The Affordable Connectivity Program, which served over 23 million households before its funding expired in 2024, was premised on the same logic as Internet Essentials: that subsidized broadband expands economic opportunity. Even after sustained federal broadband investment, county-level wired-internet adoption still ranges from 16% to 97%, with the lowest adoption concentrated in lower-income areas — the same population Internet Essentials targeted (U.S. Census Bureau and National Telecommunications and Information Administration, 2024). Our results suggest that this premise is correct for employment and credit markets, where the benefits are concrete and measurable. But broadband subsidies will not close the gap in equity market participation between low-income and higher-income households. Achieving that goal requires interventions that address the structural barriers, whether through workplace retirement savings programs with automatic enrollment (Madrian and Shea, 2001; Yogo, Whitten, and Cox, 2025), matched savings incentives, or other mechanisms that provide both the resources and the institutional structure for investment.

A caveat on interpretation is warranted here. We have described the absence of an equity response as a limit on what broadband can accomplish, but whether closing the equity participation gap is itself desirable for this population is not obvious. Kumar (2009) shows that low-income investors disproportionately hold lottery-like stocks and earn lower risk-adjusted returns as a result, and the households in our sample have little liquid wealth and substantial background risk—the conditions under which equity exposure is least likely to raise welfare. Read that way, the equity null need not be a policy failure, and households directing new resources toward precautionary savings and toward staying current on existing obligations may be operating on the welfare-improving margin. Our credit management results point the same direction: the decline in delinquency and in collection-agency activity is an unambiguous improvement in financial position, whereas the welfare consequence of moving these households into equity markets is theoretically ambiguous. We therefore read our findings as identifying where technology relaxes binding constraints, rather than as

establishing that equity participation is the right target for this population. Understanding which specific structural barriers bind, and whether relaxing them would improve welfare, is an important direction for future research.

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Table 1: Summary Statistics

Panel A: ACS Full Sample (Household Heads below 200% FPL)

	Mean	SD	5%	25%	Median	75%	95%
Employed	0.427	0.495	0	0	0	1	1
Income	7194.602	10189.143	0	0	680	13000	27800
Male	0.442	0.497	0	0	0	1	1
Age	43.982	19.618	19	27	41	58	81
Black	0.167	0.373	0	0	0	0	1
Hispanic	0.224	0.417	0	0	0	0	1
Married	0.318	0.466	0	0	0	1	1
Years of Education	11.923	3.115	6	12	12	13	16
Number of Children	0.767	1.226	0	0	0	1	3

Panel B: ACS IE-Eligible Households

	Mean	SD	5%	25%	Median	75%	95%
Employed	0.596	0.491	0	0	1	1	1
Income	12152.503	12562.072	0	0	10000	20000	35500
Male	0.374	0.484	0	0	0	1	1
Age	38.177	8.481	26	32	37	44	53
Black	0.170	0.376	0	0	0	0	1
Hispanic	0.346	0.476	0	0	0	1	1
Married	0.554	0.497	0	0	1	1	1
Years of Education	11.681	3.160	6	11	12	13	16
Number of Children	2.502	1.213	1	2	2	3	5

This table reports the summary statistics for the observations in the IPUMS ACS. Panel A reports the full sample results, which include all adult (age ≥ 18) observations in IPUMS ACS. Panel B reports results for a subsample of individuals who are eligible for Internet Essentials. The unit of observation is an individual.

Table 2: Effect of Internet Essentials on Internet Access

	Have Internet at Home				
	(1)	(2)	(3)	(4)	(5)
% Comcast $\times$ Post	0.053** (0.022)				0.047** (0.022)
% Verizon $\times$ Post		-0.028 (0.023)			-0.037 (0.030)
% AT&T $\times$ Post			0.003 (0.024)		-0.016 (0.029)
% Charter $\times$ Post				-0.058 (0.048)	-0.034 (0.046)
Year FE	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Cluster	MSA	MSA	MSA	MSA	MSA
R^2	0.146	0.146	0.145	0.146	0.146
Observations	28,953	28,953	28,953	28,953	28,953
Outcome Variable Mean	0.752	0.752	0.752	0.752	0.752

This table reports the impact of Comcast's Internet Essentials on internet access for eligible low-income households. The unit of observation is a household and is weighted accordingly using household weights (hwtfinl). Control variables include the household head's gender, age, age², race, marital status, years of education, and number of children. Standard errors reported in parentheses are clustered at the MSA level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 3: Effect of Internet Essentials on Employment and Income

	200% Poverty		150% Poverty		400% Poverty		Full Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employed	Labor Income	Employed	Labor Income	Employed	Labor Income	Employed	Labor Income
% Comcast $\times$ Post $\times$ Eligible	0.008** (0.003)	142.952** (71.868)	0.011*** (0.004)	160.864** (69.433)	0.008** (0.003)	140.430 (86.270)	0.009*** (0.003)	101.081 (110.490)
Eligibility $\times$ PUMA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Eligibility $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PUMA $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	PUMA	PUMA	PUMA	PUMA	PUMA	PUMA	PUMA	PUMA
R^2	0.165	0.202	0.145	0.191	0.212	0.284	0.240	0.328
Observations	5,055,570	5,055,570	3,651,718	3,651,718	9,985,574	9,985,574	16,536,894	16,536,894
Outcome Variable Mean	0.415	6749.366	0.374	4995.419	0.517	13089.259	0.599	24446.701

This table reports the effect of Internet Essentials on employment and earnings outcomes. The unit of observation is an individual. Control variables include the individual's gender, age, age², race, marital status, years of education, and number of children. The time window is from 2009 to 2015. The outcome variable means are measured using observations from 2011. Standard errors reported in parentheses are clustered at the PUMA level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 4: Effect of Internet Essentials on Credit Inquiries by Product Category

	(1) Collections	(2) Credit Card	(3) Personal	(4) Auto	(5) Mortgage	(6) Bank Other
<i>Panel A. Number of inquiries</i>						
% Comcast $\times$ Post $\times$ Eligible (CS)	-0.0184*** (0.0022)	0.0168*** (0.0029)	0.0019 (0.0015)	0.0062** (0.0031)	0.0046*** (0.0005)	0.0007 (0.0020)
Baseline Mean	0.2137	0.3172	0.0657	0.1408	0.0110	0.1401
% Effect	-8.6	5.3	2.9	4.4	41.9	0.5
<i>Panel B. Any inquiry (indicator)</i>						
% Comcast $\times$ Post $\times$ Eligible (CS)	-0.0073*** (0.0009)	0.0022* (0.0012)	0.0011 (0.0008)	0.0027*** (0.0009)	0.0030*** (0.0004)	0.0014 (0.0011)
Baseline Mean	0.1245	0.1791	0.0424	0.0736	0.0091	0.1055
% Effect	-5.9	1.2	2.7	3.7	33.5	1.3
% Comcast $\times$ Post Absorbed	Yes	Yes	Yes	Yes	Yes	Yes
Eligibility $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Zip	Zip	Zip	Zip	Zip	Zip
Observations	17,093,770	17,093,770	17,093,770	17,093,770	17,093,770	17,093,770

This table reports the effect of Internet Essentials on credit inquiries by product category, using the individual's average pre-treatment VantageScore (below 600) as the proxy for IE eligibility. Panel A measures the number of inquiries an individual submits in a year; Panel B measures whether the individual submits any inquiry of the given type. The columns study, in order, collection-agency inquiries, credit card inquiries, personal loan inquiries, auto financing inquiries, mortgage inquiries, and other bank credit inquiries. Baseline means are computed among eligible individuals in 2011 and are reported separately for each panel. Appendix Table IA.13 reports the same specifications using zip code median household income as the eligibility proxy. The unit of observation is an individual-year. The time window is from 2009 to 2015. Standard errors reported in parentheses are clustered at the zip code level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 5: Effect of Internet Essentials on New Credit Account Openings

	(1) New CC	(2) New Auto	(3) New Mortg.	(4) New Pers.	(5) New Coll.
% Comcast $\times$ Post $\times$ Eligible (CS)	0.0047*** (0.0007)	0.0011*** (0.0004)	0.0015*** (0.0002)	0.0000 (0.0003)	-0.0037*** (0.0012)
Clusters	32293	32293	32293	32293	32293
R^2	0.299	0.218	0.184	0.370	0.486
N	18,517,880	18,517,880	18,517,880	18,517,880	18,517,880
Baseline Mean	0.046	0.018	0.000	0.019	0.326

This table reports the effect of Internet Essentials on new credit account openings using Experian tradeline data. Each column reports results for a different product category: credit cards (column 1), auto loans (column 2), mortgages (column 3), personal loans (column 4), and collections (column 5). The outcome is an indicator for whether the individual opens a new account of the given type in the current year. Eligibility is proxied by pre-treatment credit score. The unit of observation is an individual-year. The time window is from 2009 to 2015. Standard errors reported in parentheses are clustered at the zip code level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 6: Effect of Internet Essentials on Delinquency and Credit Management

	(1) 30+ Late	(2) 60+ Late	(3) 90+ Late	(4) Derog.
% Comcast $\times$ Post $\times$ Eligible (CS)	-0.0059*** (0.0009)	-0.0046*** (0.0009)	-0.0052*** (0.0009)	-0.0011 (0.0007)
Clusters	32293	32293	32293	32293
R^2	0.777	0.770	0.764	0.832
N	18,517,880	18,517,880	18,517,880	18,517,880
Baseline Mean	0.475	0.456	0.447	0.900

This table reports the effect of Internet Essentials on delinquency and credit management outcomes using Experian tradeline data. Columns 1–3 report the likelihood of having trade-lines that are 30, 60, and 90 or more days late, respectively. Column 4 reports the likelihood of having a derogatory mark. Eligibility is proxied by pre-treatment credit score. The unit of observation is an individual-year. The time window is from 2009 to 2015. Standard errors reported in parentheses are clustered at the zip code level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 7: Effect of Internet Essentials on Savings Participation

	Have Interest	Income
	(1)	(2)
% Comcast $\times$ Post $\times$ Eligible	0.030** (0.014)	0.026* (0.013)
Eligibility $\times$ Year FE	Yes	Yes
Eligibility $\times$ MSA FE	Yes	Yes
MSA $\times$ Year FE	Yes	Yes
Controls	-	Yes
Cluster	MSA	MSA
R^2	0.072	0.134
Observations	116,097	116,097
Outcome Variable Mean	0.202	0.202

This table reports the effect of Internet Essentials on the likelihood of having savings income. The unit of observation is a household. Control variables include the household head's gender, age, age², race, marital status, years of education, and number of children. The time window is from 2009 to 2015. Standard errors reported in parentheses are clustered at the MSA level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 8: Effect of Internet Essentials on Investment Participation

	Have Investment Income		Have Dividend Income	
	(1)	(2)	(3)	(4)
	ACS	ACS	CPS	CPS
% Comcast $\times$ Post $\times$ Eligible	-0.002 (0.002)	-0.002 (0.002)	0.004 (0.007)	0.003 (0.007)
Eligibility $\times$ Year FE	Yes	Yes	Yes	Yes
Eligibility $\times$ MSA FE	-	-	Yes	Yes
MSA $\times$ Year FE	-	-	Yes	Yes
Eligibility $\times$ PUMA FE	Yes	Yes	-	-
PUMA $\times$ Year FE	Yes	Yes	-	-
Controls	-	Yes	-	Yes
Cluster	PUMA	PUMA	MSA	MSA
R^2	0.033	0.094	0.042	0.072
Observations	2,636,321	2,636,321	116,097	116,097
Outcome Variable Mean	0.078	0.078	0.040	0.040

This table reports the effect of Internet Essentials on the likelihood of having investment income. The unit of observation is a household. Control variables include the household head's gender, age, age², race, marital status, years of education, and number of children. The time window is from 2009 to 2015. Standard errors reported in parentheses are clustered at the PUMA (columns 1–2) or MSA (columns 3–4) level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

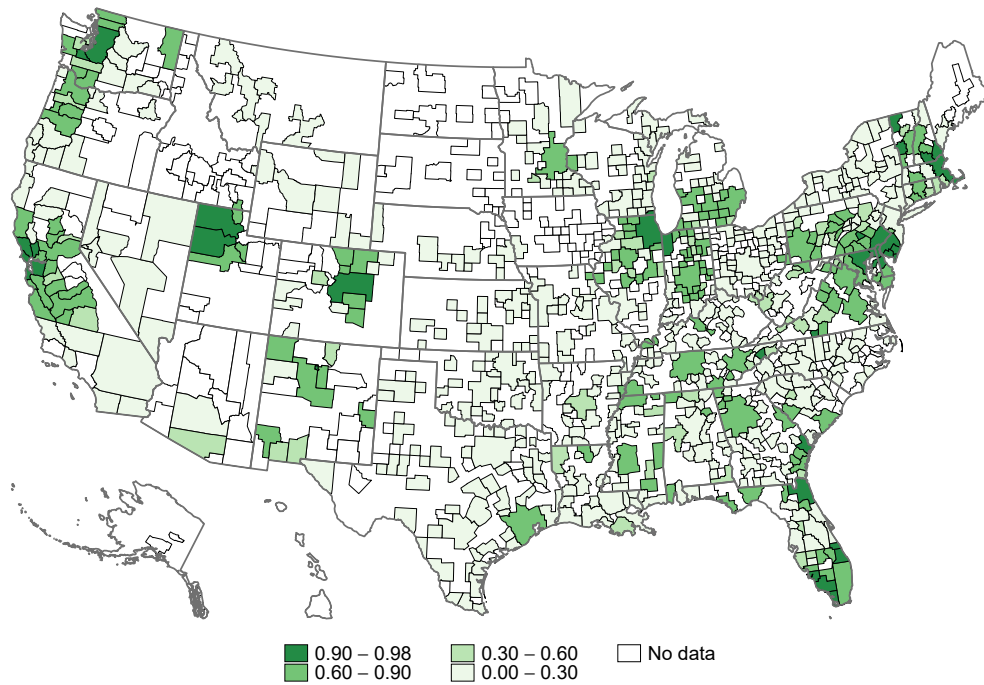


Figure 1: Comcast Coverage by MSA

This figure displays the population-weighted share of census blocks with Comcast broadband service in each Metropolitan Statistical Area, based on NTIA broadband provider data as of June 2012. Darker shading indicates greater Comcast presence.

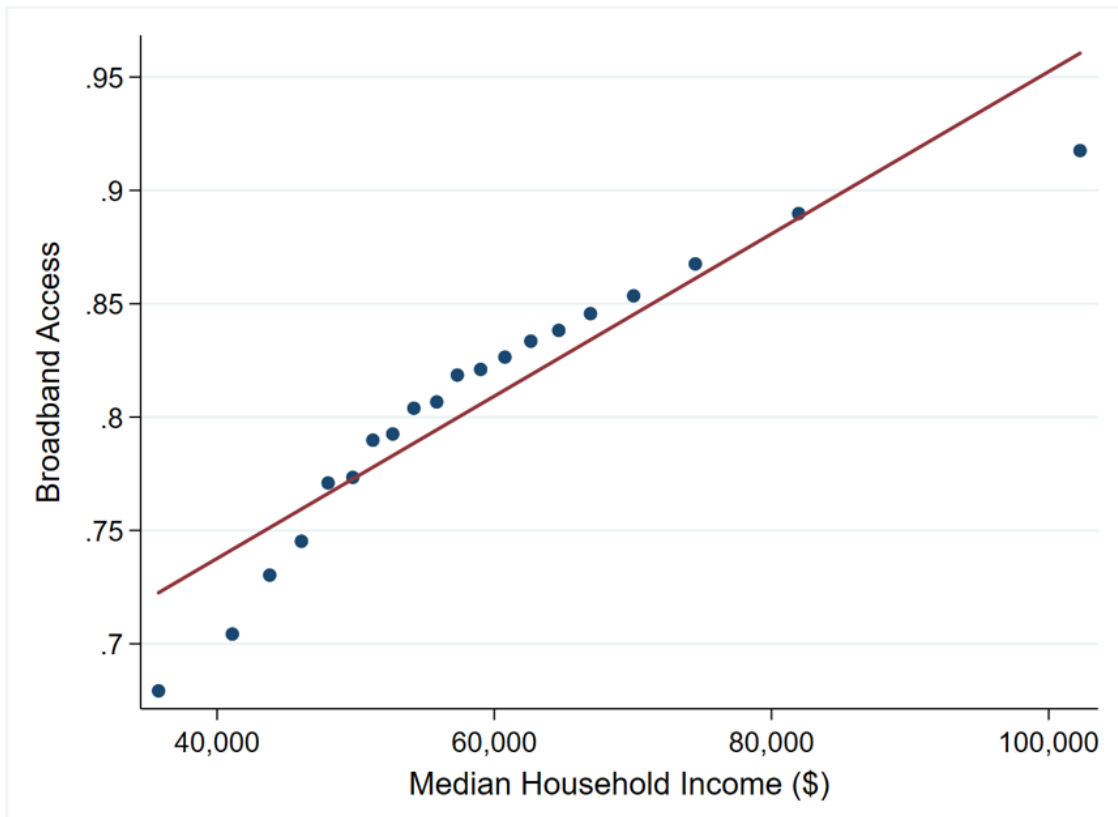


Figure 2: Internet Access by Household Income

This figure plots the share of households with broadband access by household income group using the 2023 ACS 1-year estimates.

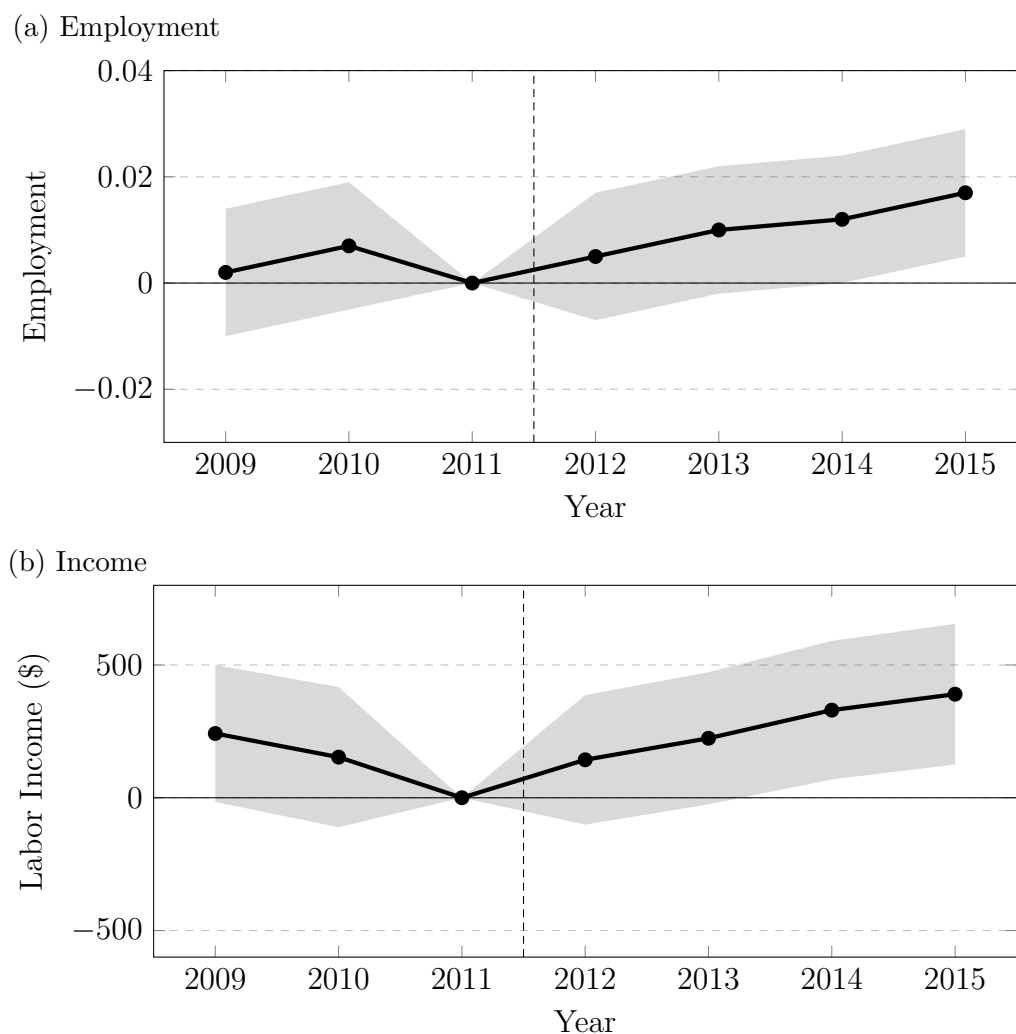


Figure 3: Event-Time Estimates: Employment and Income

Each panel plots the triple-difference coefficients by year from the ACS, with 2011 as the omitted base year. The vertical dashed line marks the 2012 IE launch. Bars represent 95% confidence intervals based on standard errors clustered at the PUMA level.

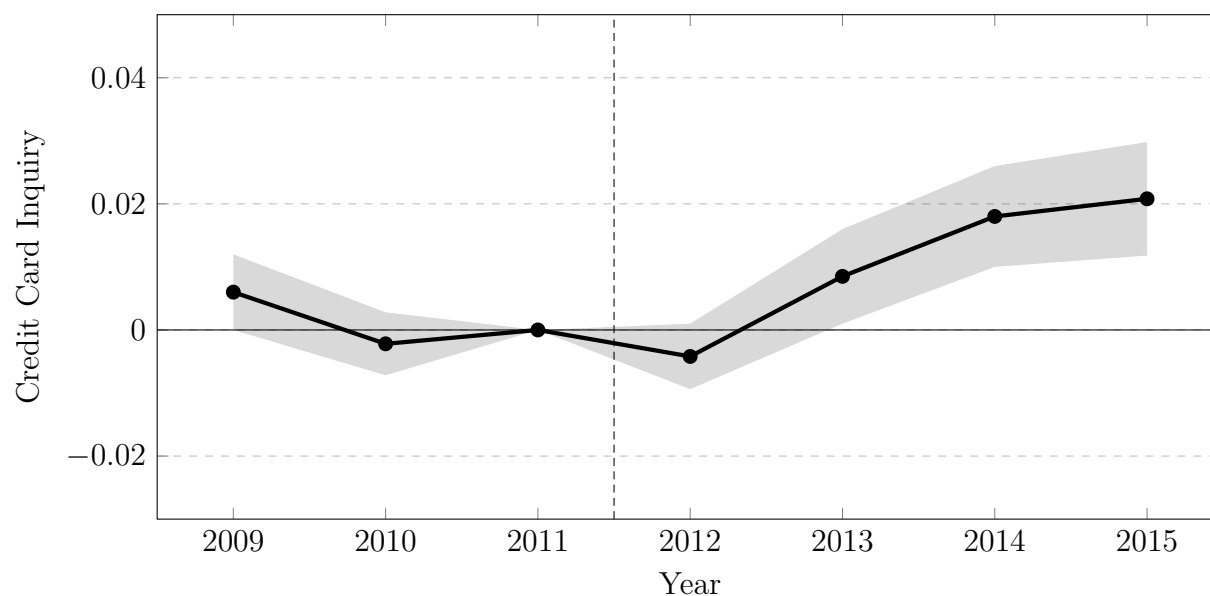


Figure 4: Event-Time Estimates: Bank Credit Card Inquiries

This figure plots event-time triple-difference coefficients for bank credit card inquiries in the Experian data, with 2011 omitted as the reference year. The outcome is an indicator for whether an individual-year has a bank credit card inquiry (KOB BC), using the credit-score proxy for IE eligibility. Estimates are shown relative to 2011, and the shaded band gives 95% confidence intervals based on standard errors clustered at the zip code level.

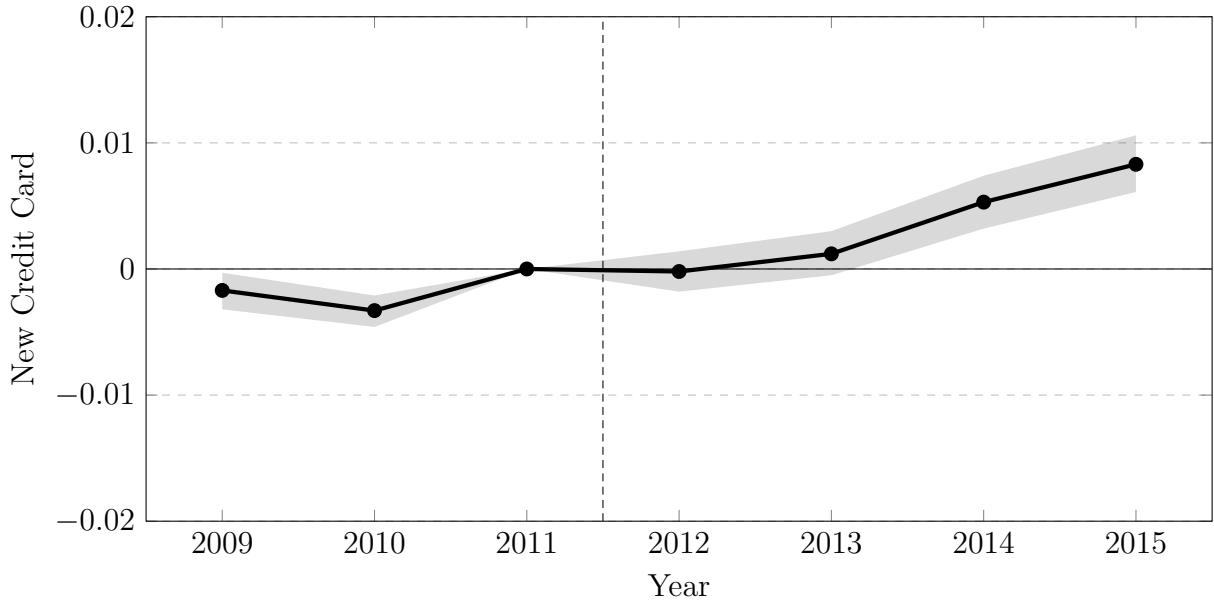


Figure 5: Event-Time Estimates: New Credit Card Account Openings

This figure plots event-time triple-difference coefficients for new credit card tradeline openings in the Experian data, with 2011 as the omitted reference year. The outcome is an indicator for opening a new credit card account in the current year, using the credit-score proxy for IE eligibility. Shaded bands represent 95% confidence intervals based on standard errors clustered at the zip code level.

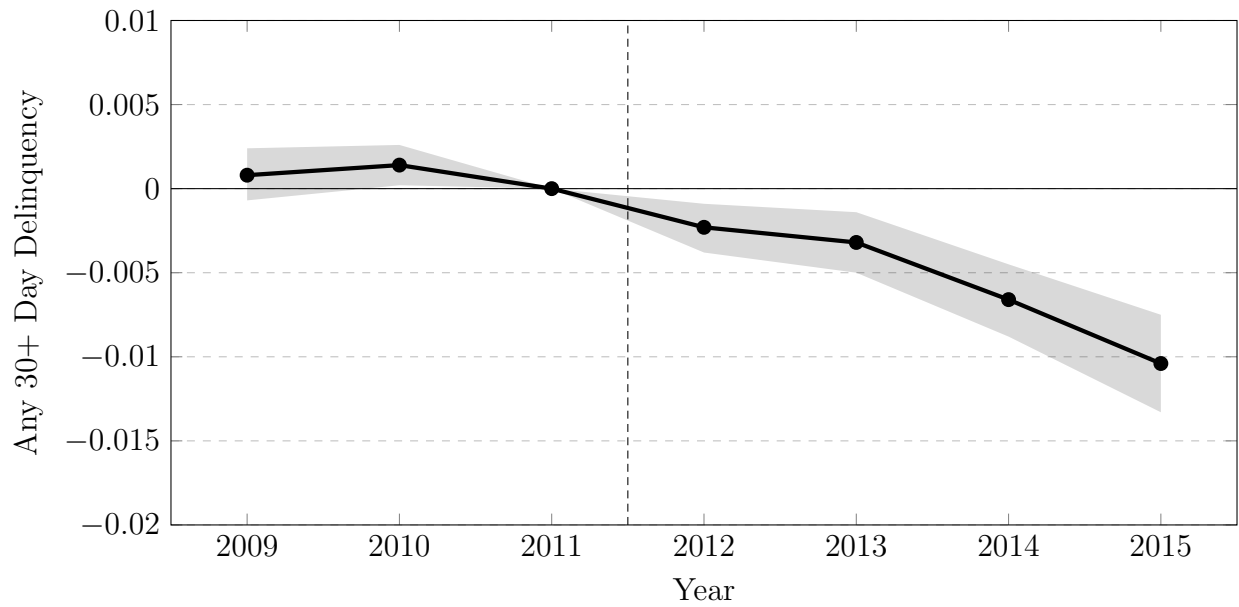


Figure 6: Event-Time Estimates: 30-Day Delinquency

This figure plots event-time triple-difference coefficients for the probability of any 30-day delinquency in the Experian tradeline data, with 2011 as the omitted reference year. The outcome is an indicator for whether the individual has any 30+ day delinquent account in the current year, using the credit-score proxy for IE eligibility. Shaded bands represent 95% confidence intervals based on standard errors clustered at the zip code level. Negative coefficients indicate improved payment behavior.

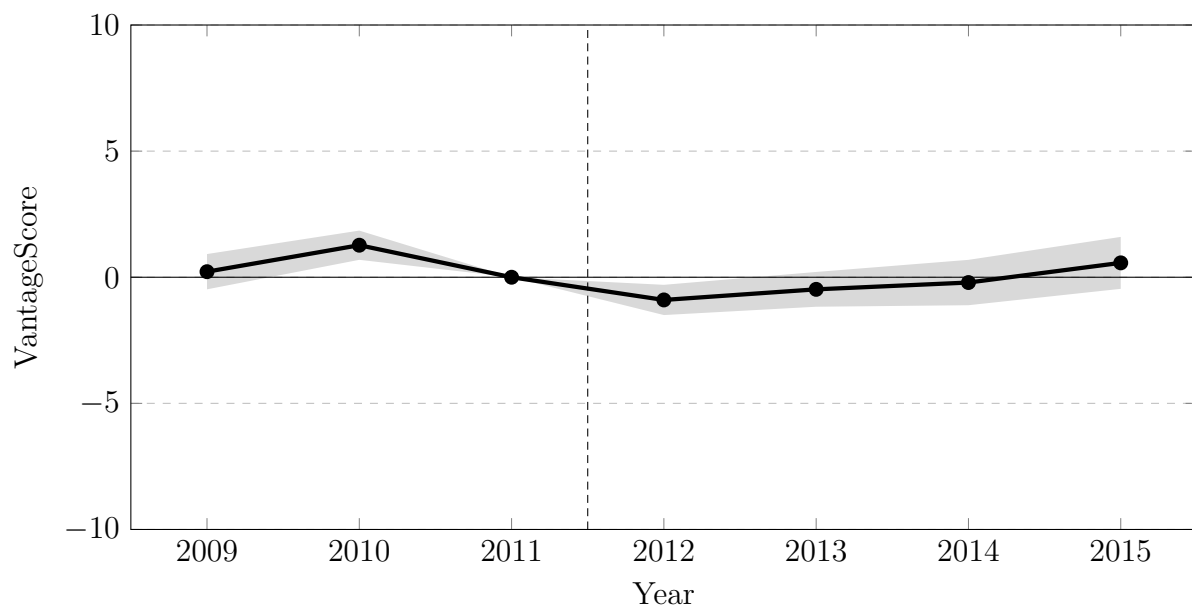


Figure 7: Event-Time Estimates: VantageScore

This figure plots event-time triple-difference coefficients for VantageScore in the Experian data, with 2011 as the omitted reference year. The outcome is the individual's annual VantageScore, using the credit-score proxy for IE eligibility. Shaded bands represent 95% confidence intervals based on standard errors clustered at the zip code level.

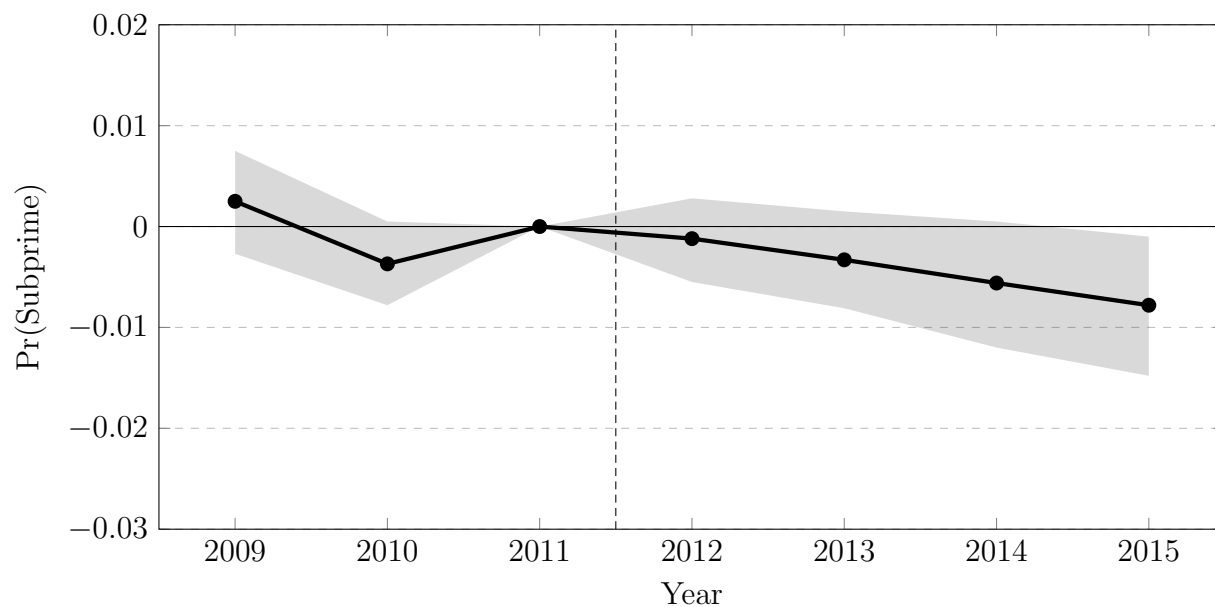


Figure 8: Event-Time Estimates: Subprime Probability

This figure plots event-time triple-difference coefficients for subprime status in the Experian data, with 2011 as the omitted reference year. The outcome is an indicator for whether the individual's VantageScore is below 600, using the credit-score proxy for IE eligibility. Shaded bands represent 95% confidence intervals based on standard errors clustered at the zip code level. Negative coefficients indicate a lower probability of subprime status.

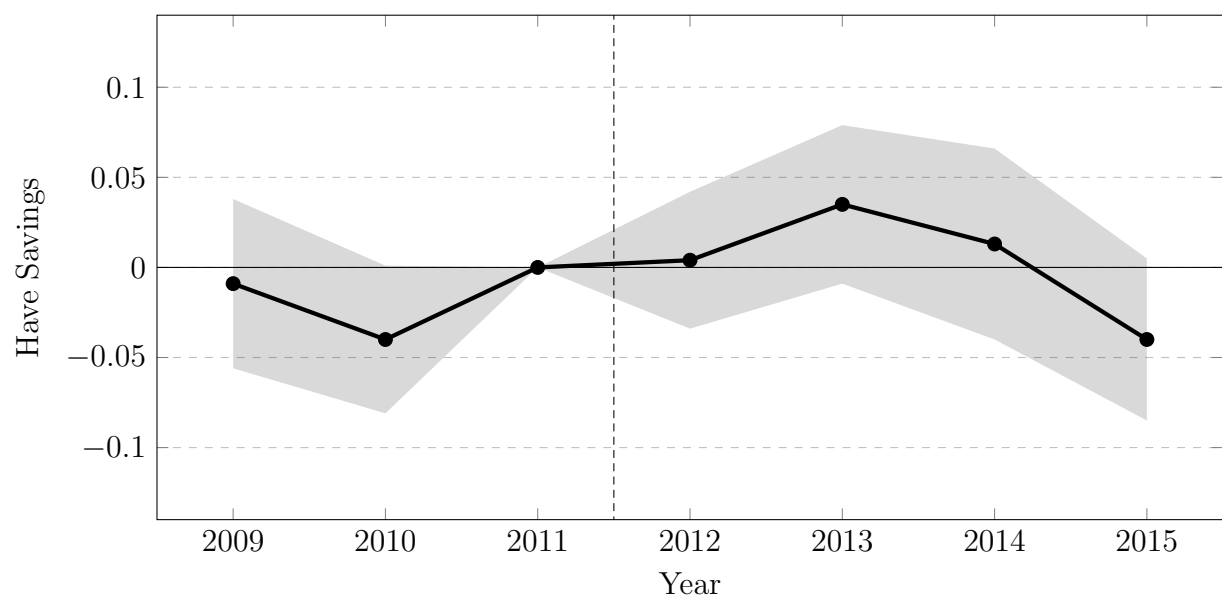


Figure 9: Event-Time Estimates: Savings Participation

This figure plots triple-difference coefficients for CPS interest income participation by year, with 2011 as the omitted base year. Bars represent 95% confidence intervals based on standard errors clustered at the MSA level.

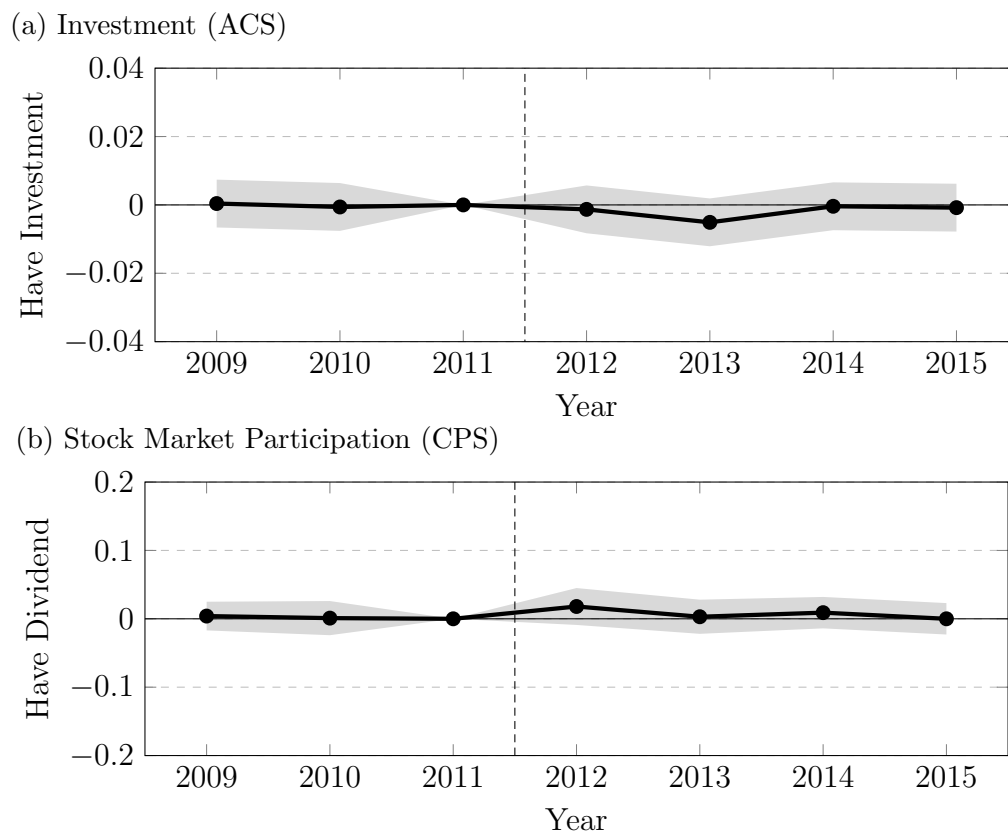


Figure 10: Event-Time Estimates: Investment Participation

Panel (a) plots triple-difference coefficients for ACS investment income participation by year. Panel (b) plots the corresponding coefficients for CPS dividend income. The year 2011 is the omitted base year. Bars represent 95% confidence intervals.

Internet Appendix Tables

Table IA.1: Results Using IRS Data

	(1) Frac. W-2	(2) Frac. EITC	(3) Frac. Int.	(4) Frac. Div.
% Comcast $\times$ Post $\times$ Eligible	0.006*** (0.001)	0.004*** (0.001)	0.008*** (0.001)	-0.001 (0.001)
Eligibility $\times$ Year FE	Yes	Yes	Yes	Yes
Eligibility $\times$ County FE	Yes	Yes	Yes	Yes
% Comcast $\times$ Year Absorbed	Yes	Yes	Yes	Yes
Cluster	County	County	County	County
R^2	0.958	0.994	0.984	0.973
Observations	37,692	37,692	37,692	37,692
Outcome Variable Mean if Eligible	0.769	0.361	0.220	0.097

This table reports the effect of Internet Essentials on household outcomes using IRS data. The unit of observation is a county-income bracket pair. The time window is from 2010 to 2015. Eligible is equal to one if AGI $\leq$ \$25,000. Standard errors reported in parentheses are clustered at the county level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table IA.2: Savings Participation: Sensitivity to Sample Period (CPS)

	Have Interest Income (Savings)		
	(1)	(2)	(3)
	Baseline	Drop 2009-10	Drop 2009
% Comcast $\times$ Post $\times$ Eligible	0.026* (0.013)	0.011 (0.020)	0.030** (0.015)
Sample Period	2009-2015	2011-2015	2010-2015
Number of Clusters	308	308	308
R^2	0.134	0.132	0.133
Observations	116,097	82,519	99,344
Outcome Variable Mean	0.202	0.202	0.202

This table reports the sensitivity of the savings participation results to the choice of sample period. Column 1 replicates the baseline specification using the full 2009–2015 sample. Column 2 drops the 2009–2010 pre-period observations, restricting the sample to 2011–2015. Column 3 drops only 2009, using the 2010–2015 sample. The unit of observation is a household. Standard errors reported in parentheses are clustered at the MSA level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table IA.3: Effect of Internet Essentials on ACS Investment Income (Interest + Dividends + Rental)

	(1)	Full Period		Drop 2009-10	
	(1)	(2)	(3)	(4)	(5)
	Any	Small	$\leq$ Med	Any	Small
% Comcast $\times$ Post $\times$ Eligible	-0.0025 (0.0017)	-0.0017 (0.0012)	-0.0016 (0.0013)	-0.0022 (0.0027)	-0.0023 (0.0019)
Outcome	Any Inc > 0	Inc $\leq$ \$500	Inc $\leq$ median	Any Inc > 0	Inc $\leq$ \$500
Sample Period	2009-2015	2009-2015	2009-2015	2011-2015	2011-2015
Clusters	1078	1078	1078	1078	1078
R^2	0.092	0.036	0.040	0.092	0.038
Observations	2,636,321	2,636,321	2,636,321	1,913,371	1,913,371
Baseline Mean	0.031	0.015	0.016	0.031	0.015

This table reports the effect of Internet Essentials on ACS investment income, defined as the sum of interest, dividend, and rental income. Columns 1–3 use the full 2009–2015 sample; columns 4–5 restrict to 2011–2015 to assess sensitivity to the pre-treatment period. Columns 1 and 4 use any positive investment income as the outcome; columns 2 and 5 use investment income below \$500; column 3 uses investment income below the sample median. The unit of observation is an individual. Standard errors reported in parentheses are clustered at the PUMA level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table IA.4: Savings Participation by Severity of Pre-Existing Banking Barriers

	Financial literacy		Unbanked share		Deposit HHI		Branch distance	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High	Low	Low	High	Low	High	Close	Far
% Comcast $\times$ Post $\times$ Eligible	0.014 (0.043)	0.026* (0.014)	0.012 (0.041)	0.027* (0.015)	0.021 (0.017)	0.041** (0.021)	0.018 (0.019)	0.038** (0.019)
<i>p</i> -value (difference)	0.776		0.731		0.453		0.458	
<i>R</i> ²	0.138	0.130	0.143	0.128	0.134	0.135	0.125	0.144
Observations	35,217	80,880	36,215	79,882	57,402	56,122	57,266	56,258
Outcome Variable Mean	0.261	0.186	0.268	0.180	0.199	0.190	0.186	0.203

This table reports the effect of Internet Essentials on savings participation separately by four proxies for the severity of pre-existing barriers to banking. The outcome is an indicator for whether the household reports any interest income. Columns 1 and 2 split the sample at the median of state-level financial literacy, measured by FINRA test scores. Columns 3 and 4 split at the median of the state-level share of unbanked households, measured by the FDIC survey. Columns 5 and 6 split at the median deposit Herfindahl-Hirschman index of the household's CBSA, and columns 7 and 8 at the median distance to the nearest bank branch in the household's CBSA, both constructed from FDIC Summary of Deposits data. A higher Herfindahl-Hirschman index indicates a more concentrated local deposit market. The two bank market structure splits use 2011 values so that the classification is unaffected by post-2012 branch closures. Within each pair, the second column is the subsample in which the barrier is more severe. The row labeled *p*-value (difference) reports a test of equality of the triple-difference coefficients across the two columns of each pair; the subsamples are disjoint and share no clusters, so the two estimates are treated as independent. All columns include eligibility $\times$ year, eligibility $\times$ MSA, and MSA $\times$ year fixed effects and the full set of household controls. The unit of observation is a household. The time window is from 2009 to 2015. Columns 5 through 8 use fewer observations than columns 1 through 4 because the FDIC data do not cover every CBSA in the estimation sample. Standard errors reported in parentheses are clustered at the MSA level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table IA.5: Endogenous Correlation between Internet Access and Investment Participation

	Have Investment Income				
	(1) Full Sample	(2) IE Eligible	(3) IE Ineligible	(4) $\leq 200\%$ FPL	(5) $> 200\%$ FPL
Have Internet	0.032*** (0.001)	0.006*** (0.001)	0.033*** (0.001)	0.028*** (0.001)	0.031*** (0.001)
ln(Family Income)	0.086*** (0.001)	0.007*** (0.001)	0.097*** (0.001)	0.022*** (0.001)	0.130*** (0.001)
PUMA $\times$ Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Cluster	PUMA	PUMA	PUMA	PUMA	PUMA
R^2	0.184	0.061	0.179	0.100	0.181
Observations	3,654,874	252,282	3,402,591	1,126,648	2,528,226
Outcome Variable Mean	0.212	0.033	0.228	0.087	0.273

This table reports the correlation between internet access and investment participation using ACS data. The unit of observation is a household. The time window is from 2013 to 2015. Control variables include the household head's gender, age, age², race, marital status, years of education, and number of children. Column 1 reports the result for the full sample. Columns 2 and 3 split the sample by Internet Essentials (IE) eligibility. Columns 4 and 5 split the sample by income below or above 200% FPL. Standard errors reported in parentheses are clustered at the PUMA level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table IA.6: Investment Participation by Financial Literacy and Employment Conditions

	Financial Literacy (CPS)		Pre-Treatment Employment (ACS)	
	(1)	(2)	(3)	(4)
	High Lit.	Low Lit.	Low Pre-Emp	High Pre-Emp
% Comcast $\times$ Post $\times$ Eligible	-0.0043 (0.0133)	0.0055 (0.0086)	-0.0057** (0.0027)	-0.0003 (0.0023)
Data	CPS	CPS	ACS	ACS
Subsample	High Literacy	Low Literacy	Low Pre-Emp	High Pre-Emp
Clusters	93	220	551	527
R^2	0.082	0.068	0.090	0.094
Observations	35,217	80,880	1,317,680	1,318,641
Baseline Mean	0.060	0.034	0.079	0.073
<i>P-value for employment difference: 0.611</i>				

This table reports the effect of Internet Essentials on investment participation across subgroups. Columns 1 and 2 split the CPS sample by high and low financial literacy, respectively. Columns 3 and 4 split the ACS sample by low and high pre-treatment employment rates, respectively. The unit of observation is a household (columns 1–2) or an individual (columns 3–4). The time window is from 2009 to 2015. Standard errors reported in parentheses are clustered at the MSA level (columns 1–2) or PUMA level (columns 3–4). Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table IA.7: Investment Participation by Income within Eligible Population

	Eligible (Poverty $\leq$ 185%)			Inelig.
	(1)	(2)	(3)	(4)
	Poorest	Middle	High Elig	Just Inelig
% Comcast $\times$ Post	-0.0007 (0.0011)	-0.0005 (0.0017)	-0.0046** (0.0022)	0.0012 (0.0026)
Income Group	Pov $\leq$ 100%	Pov 101-150%	Pov 151-185%	Pov 186-200%
N	468,015	318,274	228,481	410,744
R^2	0.052	0.048	0.047	0.120
Baseline Invest Rate	0.0183	0.0280	0.0383	0.1032

Each column reports a separate regression on the indicated poverty subgroup.

All specifications include PUMA $\times$ year, PUMA $\times$ eligibility, and eligibility $\times$ year FE.

Standard errors clustered at PUMA level.

Table IA.8: Tradeline Results with Group-Specific Linear Time Trends

	(1) New CC	(2) + Trend	(3) New Auto	(4) + Trend	(5) 30 Late	(6) + Trend	(7) Coll.	(8) + Trend
% Comcast $\times$ Post $\times$ Eligible (CS)	0.0047*** (0.0007)	-0.0025** (0.0010)	0.0011*** (0.0004)	-0.0004 (0.0006)	-0.0059*** (0.0009)	0.0003 (0.0009)	0.0038*** (0.0010)	0.0044*** (0.0011)
% Comcast $\times$ Elig $\times$ Trend		0.0023*** (0.0003)		0.0005*** (0.0002)		-0.0020*** (0.0003)		-0.0002 (0.0004)
Group Linear Trend	No	Yes	No	Yes	No	Yes	No	Yes
Clusters	32293	32293	32293	32293	32293	32293	32293	32293
R^2	0.299	0.299	0.218	0.218	0.777	0.777	0.789	0.789
N	18,517,880	18,517,880	18,517,880	18,517,880	18,517,880	18,517,880	18,517,880	18,517,880
Baseline Mean	0.0462	0.0462	0.0178	0.0178	0.4748	0.4748	0.7327	0.7327

This table reports robustness of the Experian tradeline results to adding a group-specific linear time trend. Columns 1, 3, 5, and 7 report the baseline triple-difference estimates for new credit card accounts, new auto accounts, any 30-day delinquency, and collections, respectively. Columns 2, 4, 6, and 8 add an interaction between eligibility status and a linear time trend. The outcome variables are individual-year indicators constructed from the tradeline data, using the credit-score proxy for IE eligibility. The unit of observation is an individual-year. The time window is from 2009 to 2015. Standard errors reported in parentheses are clustered at the zip code level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table IA.9: Collections Results: Robustness to Pre-Trend Controls and Sample Period

	Collections (KOB YC)	
	(1)	(2)
	Baseline	Drop 2009–10
% Comcast $\times$ Post $\times$ Eligible	-0.011*** (0.002)	-0.006*** (0.002)
Sample Period	2009-2015	2011-2015
R^2	0.435	0.513
Observations	18,610,817	13,037,727
Outcome Variable Mean	0.060	0.060

This table reports the robustness of the collections inquiry results to the choice of sample period. Column 1 replicates the baseline specification. Column 2 drops the 2009–2010 pre-period observations, retaining 2011 as the only pre-treatment year. The unit of observation is an individual-year. Standard errors reported in parentheses are clustered at the zip code level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table IA.10: Benjamini-Hochberg False Discovery Rate Corrections

Outcome	Pillar	Coefficient	Raw p -value	FDR q -value	Source
Savings participation (CPS)	Savings	0.030**	0.007	0.018	Table 5
Savings amount (CPS, log)	Savings	0.140*	0.047	0.059	Table A.4
Credit access inquiries (B)	Credit	0.026***	<0.001	0.001	Table 7
Collection inquiries (Y)	Credit	-0.027***	<0.001	<0.001	Table 7
Bank credit card (BC)	Credit	0.008*	0.046	0.059	Product decomp
Personal loans (FP)	Credit	0.005**	0.012	0.022	Product decomp
Auto financing (FA)	Credit	0.008**	0.008	0.018	Product decomp
Bank mortgage (BM)	Credit	0.005**	0.012	0.022	Product decomp
Investment income (ACS)	Investment	-0.002	0.318	0.342	Table 3
Dividend income (CPS)	Investment	0.004	0.618	0.618	Table 3
Dividend fraction (IRS)	Investment	-0.001	0.317	0.342	Table 8
Employment (ACS)	Validation	0.008**	0.008	0.018	Table 3
Labor income (ACS)	Validation	142.0**	0.029	0.045	Table 3
Internet access (CPS)	Validation	0.037**	0.002	0.011	Table 3

Total hypotheses: 14. Rejected at FDR 5%: 9. Rejected at FDR 10%: 11.

This table reports Benjamini-Hochberg false discovery rate (FDR) corrections for the 14 hypothesis tests conducted across the main results. Each row corresponds to one outcome variable. The “Pillar” column classifies outcomes by financial domain. Raw p -values are from the main specifications reported in the indicated tables. FDR q -values are computed following Benjamini and Hochberg (1995), controlling the expected proportion of false rejections among all rejected hypotheses.

Table IA.11: Robustness to Alternative Income Cutoffs for Sample Selection

	150% FPL				400% FPL				Full Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Investment	Dividend	Retirement	Interest	Investment	Dividend	Retirement	Interest	Investment	Dividend	Retirement	Interest
% Comcast $\times$ Post $\times$ Eligible	-0.001 (0.002)	0.009 (0.008)	0.023** (0.011)	0.056*** (0.015)	-0.002 (0.002)	-0.002 (0.007)	0.003 (0.013)	0.012 (0.013)	-0.001 (0.002)	-0.008 (0.008)	0.007 (0.013)	0.009 (0.013)
Eligibility $\times$ PUMA FE	Yes	-	-	-	Yes	-	-	-	Yes	-	-	-
PUMA $\times$ Year FE	Yes	-	-	-	Yes	-	-	-	Yes	-	-	-
Eligibility $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Eligibility $\times$ MSA FE	-	Yes	Yes	Yes	-	Yes	Yes	Yes	-	Yes	Yes	Yes
MSA $\times$ Year FE	-	Yes	Yes	Yes	-	Yes	Yes	Yes	-	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	PUMA	MSA	MSA	MSA	PUMA	MSA	MSA	MSA	PUMA	MSA	MSA	MSA
R^2	0.079	0.075	0.077	0.134	0.129	0.077	0.158	0.152	0.165	0.119	0.199	0.201
Observations	1,880,835	81,927	81,927	81,927	5,169,391	226,980	226,980	226,980	8,461,625	384,137	384,137	384,137
Outcome Variable Mean	0.061	0.028	0.075	0.160	0.128	0.084	0.270	0.323	0.205	0.181	0.432	0.474

This table reports the robustness of the main results to alternative income cutoffs used to define the sample. Each panel restricts the sample to household heads below 150% FPL (columns 1–4), 400% FPL (columns 5–8), or uses the full sample without an income restriction (columns 9–12). Within each panel, columns report effects on investment income (ACS), dividend income (CPS), retirement plan participation (CPS), and savings income (CPS), respectively. The time window is from 2009 to 2015. Standard errors reported in parentheses are clustered at the PUMA level (investment income columns) or MSA level (all other columns). Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table IA.12: Tradeline Results Using Zip Code Income Proxy

	(1) Any	(2) CC	(3) Auto	(4) Mortg.	(5) Pers.	(6) Coll.
% Comcast $\times$ Post $\times$ Eligible (Inc)	0.0006 (0.0024)	-0.0100*** (0.0022)	0.0007 (0.0020)	0.0004 (0.0007)	0.0082*** (0.0022)	-0.0017 (0.0034)
Clusters	31702	31702	31702	31702	31702	31702
R^2	0.811	0.826	0.619	0.849	0.715	0.793
N	19,297,385	19,297,385	19,297,385	19,297,385	19,297,385	19,297,385
Baseline Mean	0.750	0.402	0.079	0.037	0.114	0.487

This table reports the effect of Internet Essentials on tradeline account holdings using zip code median household income as a proxy for IE eligibility. Zip codes with median income below \$30,000 are classified as low-income. Each column reports results for a different product category: any tradeline (column 1), credit cards (column 2), auto loans (column 3), mortgages (column 4), personal loans (column 5), and collections (column 6). The unit of observation is an individual-year. The time window is from 2009 to 2015. Standard errors reported in parentheses are clustered at the zip code level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table IA.13: Credit Inquiry Results Using Zip Code Income Proxy (Median Income below \$30,000)

	(1) Collections	(2) Credit Card	(3) Personal	(4) Auto	(5) Mortgage	(6) Bank Other
<i>Panel A. Number of inquiries</i>						
% Comcast $\times$ Post $\times$ Eligible (Inc)	-0.0217*** (0.0046)	0.0268*** (0.0081)	0.0038 (0.0034)	0.0299*** (0.0081)	0.0079*** (0.0014)	-0.0050 (0.0050)
Baseline Mean	0.0918	0.2195	0.0382	0.0873	0.0085	0.0894
% Effect	-23.6	12.2	10.1	34.2	92.4	-5.6
<i>Panel B. Any inquiry (indicator)</i>						
% Comcast $\times$ Post $\times$ Eligible (Inc)	-0.0105*** (0.0019)	0.0084** (0.0033)	0.0051** (0.0020)	0.0080*** (0.0027)	0.0051*** (0.0011)	-0.0023 (0.0031)
Baseline Mean	0.0597	0.1362	0.0255	0.0511	0.0070	0.0703
% Effect	-17.7	6.1	19.9	15.6	73.0	-3.3
% Comcast $\times$ Post Absorbed	Yes	Yes	Yes	Yes	Yes	Yes
Eligibility $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Zip	Zip	Zip	Zip	Zip	Zip
Observations	18,610,817	18,610,817	18,610,817	18,610,817	18,610,817	18,610,817

This table repeats the specifications of Table 4 using zip code median household income (below \$30,000) as the proxy for IE eligibility in place of the pre-treatment credit score. Panel A measures the number of inquiries an individual submits in a year; Panel B measures whether the individual submits any inquiry of the given type. The columns study, in order, collection-agency inquiries (KOB YC), bank credit card inquiries (BC), finance-company personal loan inquiries (FP), auto financing inquiries (FA), bank mortgage inquiries (BM), and other bank credit inquiries (BB). The final column collects bank-group inquiries that are neither credit cards nor mortgages; because the file records only hard credit inquiries, it does not capture the opening of checking or savings accounts. Baseline means are computed among eligible individuals in 2011 and are reported separately for each panel. Estimates are larger under this proxy than under the credit score proxy in every product category. The two proxies select different populations: the credit score proxy identifies individuals who are credit-constrained as well as likely low-income, and these individuals already submit substantially more inquiries at baseline. The unit of observation is an individual-year. The time window is from 2009 to 2015. Standard errors reported in parentheses are clustered at the zip code level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table IA.14: Sensitivity of Experian Results to Credit Score Eligibility Threshold

Credit Score Cutoff	<i>N</i> Eligible (2011)	Credit Card (BC)		Collections (YC)	
		Coef.	SE	Coef.	SE
< 500	64,421	0.0096***	(0.0033)	-0.0111***	(0.0029)
< 550	414,795	0.0017	(0.0014)	-0.0092***	(0.0013)
< 600 (baseline)	828,091	0.0028**	(0.0012)	-0.0091***	(0.0010)
< 650	1,249,644	0.0006	(0.0011)	-0.0070***	(0.0007)
< 700	1,604,673	0.0002	(0.0011)	-0.0045***	(0.0006)

Each row reports the DDD coefficient from a separate regression using the indicated pre-treatment VantageScore threshold to define the credit score eligibility proxy. The baseline threshold (VantageScore < 600) is used in the main text. All specifications include eligibility $\times$ year, individual, and zip code fixed effects, with standard errors clustered at the zip code level. *N* Eligible reports the number of individuals meeting the threshold in 2011.